FEB 24 1931

Vol. 28, No. 2 PSYCHOLOGICAL REVIEW PUBLICATIONS February, 1931

# Psychological Bulletin

EDITED BY

EDWARD S. ROBINSON, YALE UNIVERSITY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
SAMUEL W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exp. Psych.*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index*)
RAYMOND DODGE, YALE UNIVERSITY (*Monographs*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY, *Business Editor*

*WITH THE CO-OPERATION OF*

G. W. ALLPORT, HARVARD UNIVERSITY; J. E. ANDERSON, UNIVERSITY OF MINNESOTA; C. M. DISERENS, UNIVERSITY OF CINCINNATI; W. T. HERON, UNIVERSITY OF MINNESOTA; J. A. McGEOCH, UNIVERSITY OF MISSOURI; K. S. LASHLEY, UNIVERSITY OF CHICAGO; J. T. METCALF, UNIVERSITY OF VERMONT; R. PINTNER, COLUMBIA UNIVERSITY.

## CONTENTS

*General Reviews and Summaries:*

*Physiological Psychology of Reading:* MILES A. TINKER, 81.
*Visually Determined Reactions in the Vertebrates:* ROSS STAGNER, 99.

*Special Reviews:* 130.

*A Note on the Relation between Brain Injury and Retentiveness:* ARTHUR W. MELTON, 175.

PUBLISHED MONTHLY (EXCEPT AUGUST AND SEPTEMBER)
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.

Entered as second-class matter at the post-office at Princeton, N. J., with an additional entry at Albany, N. Y.

# PUBLICATIONS

of the

# American Psychological Association

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
RAYMOND DODGE, YALE UNIVERSITY (*Monographs*)
EDWARD S. ROBINSON, YALE UNIVERSITY (*Bulletin*)
S. W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA (*J. of Exp. Psych.*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index and Abstracts*)
HENRY T. MOORE, SKIDMORE COLLEGE (*J. Abn. and Soc. Psychol.*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY, Business Editor

WITH THE CO-OPERATION OF

MANY DISTINGUISHED PSYCHOLOGISTS

**PSYCHOLOGICAL REVIEW**

contains original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

**PSYCHOLOGICAL BULLETIN**

contains critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

**JOURNAL OF EXPERIMENTAL PSYCHOLOGY**

contains original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 500 pages.

**PSYCHOLOGICAL INDEX**

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in June, and may be subscribed for in connection with the periodicals above, or purchased separately.

**PSYCHOLOGICAL ABSTRACTS**

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

**PSYCHOLOGICAL MONOGRAPHS**

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

**JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY**

appears quarterly, April, July, October, January, the four numbers comprising a volume of 448 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

**ANNUAL SUBSCRIPTION RATES**

**Review**: $5.50 (Foreign, $5.75). **Index**: $4.00 per volume.
**Journal**, $6.00 (Foreign, $6.25). **Monographs**: $6.00 per volume (Foreign, $6.30).
**Bulletin**: $6.00 (Foreign, $6.25). **Abstracts**: $6.00 (Foreign, $6.25).
**Abnormal and Social**: $5.00 (Foreign, $5.25). Single copies $1.50.
Current numbers: **Review** or **Journal**, $1.00; **Abstracts**, 75c; **Bulletin**, 60c.

**COMBINATION RATES**

**Review and Bulletin**: $10.00 (Foreign, $10.50).
**Review and J. Exp.**: $10.00 (Foreign, $10.50).
**Bulletin and J. Exp.**: $11.00 (Foreign, $11.50).
**Review, Bulletin, and J. Exp.**: $15.00 (Foreign, $15.75).
**Review, Bulletin, J. Exp., and Index**: $18.00 (Foreign, $18.75).

---

Subscriptions, orders, and business communications should be sent to the

## PSYCHOLOGICAL REVIEW COMPANY

## PRINCETON, N. J.

Vol. 28, No. 2 February, 1931

THE
PSYCHOLOGICAL BULLETIN

# PHYSIOLOGICAL PSYCHOLOGY OF READING

BY MILES A. TINKER
*University of Minnesota*

The investigations which are covered in this review may be grouped as follows: (1) legibility of print; (2) eye movements in reading; (3) visual apprehension;[1] (4) perception in reading. In 1927 studies on legibility and eye movement in reading (86) and in 1929 those on visual apprehension and perception in reading (87) were reviewed. Publications of the succeeding period up to April, 1930 are considered in the present paper.[2]

Bibliographies, summaries of part of the field, textbook surveys, discussions and comments, and reprinting of material that has appeared earlier will be found in references: 3, 4, 5, 6, 8, 9, 10, 25, 34, 38, 39, 40, 41, 42, 45, 52, 65, 70, 71, 93, 104, 105. Several new devices have been constructed for use in reading investigations. These are described in references: 17, 18, 21, 58, 62, 103, 110.

## *Legibility of Print*

As no decisive trends were discovered by Gould, Raines and Ruckmick (86),[3] Burtt, Beck and Cambell (14) conducted a further investigation concerning the legibility of backbone titles. The

[1] Experiments on visual apprehension will be included since the span of visual apprehension for certain types of material appears to be intimately related to perception in reading.

[2] Certain references that were not available when the earlier reviews were prepared will be included here. Some of the less recent ones will not be discussed however.

[3] Whenever references which were reviewed in the earlier reports (86, 87) are referred to, the review article will be cited rather than including such references in the present article.

two styles of printing backbone titles in common use are for reading downward with letters 90° from normal and for reading upward with letters 90° from normal. Of the two arrangements, the second was consistently more legible. The study demonstrated further that reading downward with letters in normal position is comparatively very legible. This last style of printing is not widely used.

There have been several recent investigations with regard to the legibility of unit characters. On examining more than 135,000 written digits from various sources Newland (63) discovered that figure 5 was most illegible. Next to 5 stood 7 and 2. In general, illegibilities decreased from individuals in grade III to grade IX, but from grade IX to adults there was an increase again. In a similar manner Pressey and Pressey (75) analyzed three thousand illegibilities of handwriting. They found that the capitals cause little trouble. Of the lower case letters, r and n are most illegible. Four other letters (e, a, d and o) account for many errors. About 25 per cent of the illegibilities are due to confusions among a small number of the written symbols. On the basis of these results a chart (74) was constructed for the diagnosis of illegibilities in handwriting.

Cowan (22) emphasizes the need of standardizing test cards, characterizes test letters which comply with the physiological requirements of a visual test object (23), and describes a visual test card (24) that fulfils these requirements. Letters readily recognized by their characteristic form should be avoided in constructing test cards. A modified form of the Gothic letters B C D E H O R U and S is advocated.

Working with various combinations of black, white, and gray contrast, Ferree and Rand (31, 32) determined the duration of exposure necessary for recognition of test objects (broken ring). The order of legibility[4] (quickest to slowest discrimination) is white test object on black background, black on white, white on gray, black on gray (least legible). For small sizes of test objects (1 minute) and for low intensities of illumination legibility is greater for black on white than for white on black. Physiological induction or contrast, and positive after-images are employed to explain the above phenomena.

In a somewhat similar experiment but with a normal reading situation, Starch (82, 83) ascertained the natural rate of reading

[4] The authors do not use the term "legibility" but the reviewer considers that speed of vision in this situation yields a measure of legibility.

(1) white type on a dark gray background, and (2) black type on a white background. There was a difference of 42 per cent in favor of the black type on white background. A series of Minnesota studies, not yet published, will throw more light on the apparent discrepancies between the results of Ferree and Rand, Starch, and Kirshmann (86).

Luckiesh (55), investigating the legibility of colored letters on colored backgrounds, demonstrated that legibility depends upon the relation of color to background and that the all important factor is brightness difference.

Tinker (91), comparing the orders of legibility established in various investigations, discovered a wide variation in the results for capital letters. Correlations between orders range from —.58 to +.88. With orders of legibility for lower case letters, however, there is greater agreement, the correlations ranging from +.44 to +.88. In the experimental part of this study the author determined the relative legibility of the letters, the digits, and of certain mathematical signs. In comparison with the letters, digits were found to have fair legibility, and mathematical signs only fair or poor legibility. Factors which appear to influence legibility of isolated characters are: (1) size, (2) simplicity or complexity of outline, (3) width of stroke and heaviness of type face, (4) shading and hair lines, (5) area of white space included within outline, (6) emphasis or lack of emphasis on differentiating parts. Statistical comparisons revealed that some of the most frequently used letters as the e and a are of very poor legibility.

In an effort to determine whether the confusions existing between certain letters (errors due to poor legibility) enter into the reading of words and sentences, Vernon (106) studied the reading of *typewritten* material and also analyzed part of Pyke's data (86). There was a pronounced tendency for typographical errors to occur in perceiving disconnected words, but only a slight tendency with longer sentences. The letters f and t, l and t, o and e were most commonly confused. For highly skilled readers there were few typographical errors in "real reading." The general conclusion drawn was that typographical errors are few in tachistoscopic reading and would be negligible in normal reading. Confusion of individual letters is considered to be a minor factor. Apparently the author has considered mainly the results from a few skilled readers in arriving at his final conclusions which do not coincide with facts cited under results, for earlier in his discussion Vernon admits that

where there are many typographical errors, it "is in the reading of not very extensively educated persons, and also, probably, of children." Vernon is evidently ignoring results typical of a large per cent of our reading population. The most serious defect of Vernon's study is that the stimulus material was *typewritten* but he attempts, nevertheless, to apply his findings to *printed* matter as if the two were identical. His results (not conclusions) can be accepted as valid for typewritten material and nothing else.

Chou (19), investigating the reading and legibility of Chinese symbols, states that the position of the characters is a more important factor in determining the speed of reading than the direction of reading. The temporal-spacial sequence of the "characters-in-group," arising from interaction of reading direction and character position (sequence of eye movements from one side of one character across to adjacent side of next character) "is the most important conditioning factor." Any investigation of comparative efficiency of horizontal vs. vertical reading in those who have already been dominated by one particular temporal-spacial sequence is considered fruitless.

Working with Intertype Ideal News Face, Hovde (47, 48) investigated the legibility of 6, 6½, 6¾, 7, 7½ and 8 point type with 0, ¼, ½, ¾ and 1 point leading. Line length was held constant at 12½ picas. The subjects were instructed not to hurry, but to read *normally* and for *content*. Changes in size of type and leading within the above mentioned limits apparently exerted no consistent influence on speed of reading. The author concluded that context is more important in its effect on legibility than the typographical arrangement. This last statement should be limited in its application to the conditions of Hovde's experiment. Because the reading selections were not equated objectively for difficulty and because the subjects were instructed to avoid speedy reading, Hovde's results must wait further experimentation for adequate interpretation. Paterson and Tinker (66) have demonstrated that only very slight legibility differences are to be expected between 6 and 8 point type. The reviewer considers that a speed test is essential to reveal such slight differences. It is admitted, however, that there may be no significant legibility differences within the limits here investigated.

At Minnesota three initial studies in an extensive series of investigations on typographical factors influencing speed of reading (legibility) have been published. In studying the influence of type form on speed of reading with a carefully controlled technique,

Tinker and Paterson (95) discovered that lower case Roman text was read 2.8 per cent faster than italic text, and lower case was read 13.4 per cent faster than material in all capital letters. In an earlier study Starch (82, 83) had found that italic text was not read as rapidly as Roman text. His experiment also revealed a difference of 10 per cent in favor of the lower case text in comparison with all capitals.

With length of line constant at 19 picas Paterson and Tinker (66) found that 10 point type yielded fastest reading when 6, 8, 10, 12, and 14 point type were compared. The order from fastest to slowest reading was 10 point, 8 point, 12 point, 6 point and 14 point.

With size of type constant, Scott (80) compared the speed of reading a time table printed in light and in heavy face type. The light face type produced 41 per cent more errors and was read 13.5 per cent slower than the heavy face type.

Tinker and Paterson (96), seeking an optimal line length for 10 point type, compared the reading of lines 14, 19, 23, 27, 32, 36, 40 and 44 picas long. Of these, the 19 pica (80 mm.) line was read fastest. It is possible that several line lengths in the immediate neighborhood of 19 picas are equally efficient. In comparing lines 9, 17, and 30 picas long Starch (82, 83) found that the 17 pica line produced fastest reading. He considers the optimum line length to be about 18 picas.

### *Eye Movements in Reading*

The peep-hole method (58) is recommended by Miles and Segal (59) for clinical observation of eye movements in investigating the reading habits of children. They found the technique adapted to observing fixations, regressions, confusions (detailed examination of a word), head movements and vocalization all at once. A quantitative study of these factors in the reading of 59 third grade students revealed a clear differentiation of good and poor readers. For the whole group there were, on the average, 3.8 fixations and 0.2 regressions in selection A, and 4.7 fixations and 0.8 regressions in B. In an earlier experiment and with a smaller group, Buswell (86), employing a photographic method, found medians of 8.9 fixations and 1.8 regressions for third grade children. The two investigations did not use the same reading material. While use of direct observational methods may allow some eye movements to go undetected, the Miles technique is valuable for detecting very good

and very poor readers in comparison with the average. Also, as Tinker (89) has shown, group comparisons demonstrate the same trends when made by means of a direct observation method or by the photographic technique.

Miles and Bell (60) have evaluated the use of photographic records of eye movements in investigating study habits. On the basis of results from 16 subjects the authors suggest that by using three or more measures of studying ability, as Thorndike intelligence test scores, amount read in rapid reading practice, and eye movement records, it is possible to make a practical diagnosis of the studying ability of college students.

The relative validity of photographic measures of reading ability has been investigated by Tinker (94). Photographic measures of reading ability yield number of pauses per line, number of regressions per line, pause duration, and perception time per line. Perception time is the most valid photographic measure of speed of reading. In reading easy narrative, pause frequency is a very good measure, pause duration a good measure, and regression frequency a fair measure of reading performance. With scientific prose, however, regressions have little diagnostic value and pause duration none. Changes in the reading situation favor inconsistency in reading performance.

A group of studies have investigated eye movements in special types of reading. Abernethy (1) discovered that eye movements in studying spelling are analogous to those in other kinds of reading. The more mature spellers have a broader recognition span, briefer fixational pauses, and make fewer regressions. Eye movements of good spellers tend to be systematic. Adults survey words as wholes while children exhibit random eye movements in studying spelling.

An important experimental study of eye movements in reading English (vernacular), French, German, Spanish and Latin is reported by Buswell (15). At the end of two years training, elementary school pupils have not reached the maturity in reading (efficient eye movements) a foreign language attained by high school and college students after a like period of study. The direct method of teaching produces desirable reading habits but translation methods do not. Students with two years of training exhibit similar motor habits in reading French, Spanish and German but less efficient eye movements in Latin. The average student does not achieve mature reading habits at the end of two years of studying a foreign language.

With two different methods (photographic records and direct observation of eye movements) Tinker (89) obtained consistent results demonstrating that the compactness of printing achieved by employing numerals rather than words, produces faster reading of arithmetical problems.

An extensive photographic study of eye movements in reading formulae is described by Tinker (90). Four phases of the problem were investigated: (1) the reading of algebraic formulae in context makes a much greater demand on the eye in frequency of fixations and regressions per line than the reading of either scientific prose or algebraic narrative. (2) Occular performance in reading chemical formulae in context by mature students is very similar in nature to that in reading algebraic formulae in context. Eye movement records of elementary students, however, revealed various modes of reading the formulae. They read some formulae and omitted others. (3) The reading of isolated formulae was essentially the same in nature for readers of various amounts of training. This reading was mainly analytical in nature and was characterized (a) by a comparatively large number of fixations per line, (b) by comparatively long fixational pauses, (c) by many regressions, and (d) by the bunching of fixations about fractions and compound exponents. (4) Length of eye movements was less, and more of the line was covered by eye fixations in lines of material involving formulae than in those without formulae. The experiment as a whole demonstrated that reading formulae is quite different from reading prose both in the motor phases and in the mental processes involved. These results, together with Georges' (36) analysis of difficulties encountered in reading mathematics suggest the need of special instruction in reading mathematical material.

Pressey (76) has developed a test involving the observation of eye movements for diagnostic use. A manual by the same author (77) contains exercises designed to promote more efficient eye movements in reading.

A few experiments have been concerned with more technical aspects of the saccadic eye movements involved in reading. Leiri (54) demonstrated that ordinarily no visual impressions enter consciousness during the period of saccadic eye movements. This had been previously demonstrated by Dodge (86) and Holt (46). Leiri (like Dodge) noted that under certain conditions vision was possible during the movements. The failure of perception during saccadic eye movements under most conditions is explained as due to "cen-

tral anaesthesia." Leiri suggests that in ordinary circumstances this failure of perception is due to the strong foveal impressions received when the eye is fixated which prevent the visual impressions received during rapid eye movement from reaching consciousness. This interpretation agrees more with Dodge than with Holt.

According to Vernon (**107**) subjective impressions of ease or difficulty are closely connected with accuracy and inaccuracy of performance in saccadic eye movements from right to left of the visual field. He found accuracy to be greatest when the deeply rooted motor habits of normal reading were operative.

The relationship between eye movement duration, pause duration and reading time while reading various types of material was investigated by Tinker (**92**). On the average 5.9 per cent of the reading time was consumed by eye movements. This allowed about 94 per cent for perception, i.e., pauses. It was found that the more careful and analytical the reading the smaller the ratio between eye movement duration and reading time.

### *Visual Apprehension*

Recent studies of visual apprehension are concerned with various factors which influence perceptual span. Working with green, blue, red, and yellow colored stimuli Brown (**12**) discovered that heterogeneity of chromatic stimuli tends to reduce the magnitude of the limens (spans). Yellow appeared to be an indisputable factor causing this reduction. Nixon (**64**), comparing advertising and non-advertising material, found that the presence or absence of color had little or no effect on the quickness with which a copy was recognized as an advertisement.

Heigl (**43**) found no definite and consistent effect of tea on perceptual span, but Ach (**2**) discovered that caffein produces increased efficiency in perceiving words and nonsense syllables. The greater efficiency was manifested in quicker and more accurate apprehension.

When intervals of two to sixty seconds had elapsed between presentation of stimuli and response, Wartensleben's (**108**) subjects gave practically the same performance at all intervals. The subjects recited to themselves or retained a visual image of stimuli during the interval. These results do not agree with those of Finzi (**86**) which indicated that a four-second interval was optimum.

Tinker, Roberts and Jackson (**97**) obtained results in the visual apprehension experiment which led them to conclude that the use

of either definite or indefinite preparation yields equally efficient apprehension when the time of exposure is approximately 1/10 sec., the definite preparation is two sec., and the indefinite preparation varies between the limits of two and thirty-two sec.

Employing experimental situations which involved: (1) merely indicating the number of objects exposed; (2) reproduction of letters exposed, and (3) giving the number, the name and the qualities of stimuli exposed, Glanville and Dallenbach (37) found that the range of visual apprehension varied inversely with the degree of cognition required in the report. With a high degree of cognition the range was small.

In Judd's experiment (50), which required the subjects to apprehend the number of stimuli presented, the errors increased with (1) the larger series, (2) more than 4 sounds per sec., (3) more than 3 flashes of light per sec., or (4) with more than 2 taps on the hand per sec. Sounds were easier to count than flashes of light while taps on the hand were hardest to apprehend correctly.

An important investigation of apprehension of numbers by children is reported by Brownell (13) in which an adaptation of the technique employed by Howell (49) and Lay (53) is used. The stimuli consisted of dots arranged in the following manner: square (4 dots per group); diamond (4 dots); domino (5 dots); triangular (3 dots); odd; and linear. Children from grades I to VII estimated the number of dots in series exposed for 5 sec. Some of the more important results follow: (1) When grouping is eliminated, the difficulty of apprehending visual concrete numbers is in direct proportion to the number of objects exposed. (2) Mature methods of apprehension (use of grouping to increase efficiency) is not common before the third and fourth grades and is not universal in the fifth. (3) I.Q. appears to be positively correlated with accuracy of apprehension.

In Pyle's experiment (78) with children as subjects the spans of visual apprehension for words in sentences reveal gradual increases with age and grade up to 13 years and grade VIII. The girls were slightly superior to boys at all levels. Span appears to be somewhat related to intelligence and reading ability.

A somewhat similar but more intensive study by Payne (68) derived tentative norms for short exposure reading, using children in grades II to V. In general, there was an increase in perceptual span with both increase in age and grade.

Span of visual apprehension for characters in formulae was

found by Tinker (88) to be almost twice as great as for unrelated letters. This larger span indicates a perceptual tendency toward configuration or grouping in reading the formulae.

Flash card exercises are much used as a supplement to ordinary reading. Gates (33) has attempted to evaluate this practice in the first four grades. Intensive training increased ability to read flash cards quickly and accurately and hence these exercises may be useful in their own right. However, there appeared to be no transfer to the type of reading measured by silent and oral reading tests.

Several studies have been concerned with factors influencing judgment of numbers. The results of Ponzo (72, 73) indicate that judgment of a group of similar elements (dots, circles, or lines) is dependent on such factors as the actual or apparent size of the design and the shape and groupings of the elements. De Machi (29), seeking to determine the main factors influencing numerical estimates, varied the duration of exposure, the size of area within which the stimuli appeared, etc. The trend of results made it possible to divide the subjects into those who over-estimated and those who under-estimated the groups of stimuli.

Studying the applicability of 6-place numbers to automobile plates, Dolezal (30) shows that, with 2 sec. exposure, only 70.5 per cent are recognized correctly; and with 1 sec., only 51 per cent.

### *Perception in Reading*

Stein (84) is concerned mainly with an analysis of word form in reading. Form quality was not destroyed by successive presentation of a word by letters, by backward presentation of words or omission of letters. Word form in perception proved to be most effective with simultaneous presentation of the material with the fixation at the left on the beginning of the exposed word. Although the importance of total word form is emphasized, reading may occur without total form in some situations. In a related experiment, Thorner (85) determined the influence of various arrangements of letters on perception. While meaningful vs. nonsense words is one factor in word perception, this experiment demonstrates that other important determinants are (1) dominance (letter groups including letters as p and t), and (2) pronounceability of letter groups. This experiment was extended to the reading of meaningful material by Heimann and Thorner (44). Familiar and unfamiliar words were read in both capitals and lower case letters. Analysis of the results indicate that total word form normally plays the most important

role in word perception. In more difficult words, however, there is a greater tendency for the single letters to influence apprehension. These studies serve to further emphasize the importance of word form as a factor in adult reading.

Pickford (69) attempted to determine the relation of meaning (context) to perception but due to an unfortunate choice of stimulus material equivocal results were obtained.

By reading Chinese in various arrangements Chou (20) sought to ascertain whether there is a sentence form operative in reading a line of Chinese characters comparable to word form in English. He concluded that the reading of Chinese involves sentence form and this is defined as the temporal-spacial sequence of the Chinese words (progress in perceiving from one side of a character to the adjoining side of the next) in the line irrespective either of the reading direction or of the character position. In this same report Chou states that "the time required to do a given amount is a much more adequate and accurate measure than the amount done in a given time." The author has in mind the measurement of reading speed. Furthermore, in another report (18), describing a new technique for reading reactions, Chou upholds the same view that the work-limit method is superior to the time-limit method. Experimental results fail to substantiate Chou's statement, for Paterson and Tinker (67) have shown that the work-limit and time-limit methods are equivalent in short speed tests of reading.

Recent classifications of reading types correspond in some degree with those previously reviewed (87). Thorner (85) divides his readers into visual and auditory types. Vernon (106) noted reading types that correspond to the objective and subjective types of Messmer (87), and also distinguished in addition a broad-inaccurate and a narrow-inaccurate type. The classification of Dearborn (27) includes 3 types: (1) Part-word readers (parts of words serve as cues); (2) Word form readers (word form and determining letters important); (3) Mixed class (use parts of words or word form for different words).

For American students the difficulty of learning Chinese characters for recognition is independent of the complexity of the characters according to the results of Tsai and Abernethy (98). For reproduction, however, difficulty of learning increases with the complexity of the characters. In recognition, the subjects react to "characters-as-a-whole" while in reproduction subjects react first

to some salient feature of the word, and only to the rest when greater mastery of the character is achieved.

Word perception by children has been studied in many investigations. Using the technique employed by Gates (87), and with children as subjects, Sister Mary (56) found that perception does not operate uniformly with all kinds of material and in every situation. She states that ability to discriminate minute details in series of actual words (not in series of digits or drawings) and also in groups of unrelated letters are preeminent factors in reading.

Bowden's results (11) seemed to justify the conclusion that although in children's reading, word form is very important, the ability to perceive details is also an essential factor. It may be the word length, the first letter, the last letter, a characteristic letter, the position of a word in the sentence, or even the blackness of the type that serves as the cue. To Gates and Boecker (35) however, word form seemed of less importance in the reading of children. Their results indicate that children learn words frequently by observing some minute detail such as the dot over an i or the tail of a y. Long words were harder to learn than short ones. Similarly, Meek (57) considers word form to be of less importance than the characteristic form of letters or groups of letters in the perceiving of words by children. Wiley (109) states that children often react to words on the basis of slight and inadequate cues, and that configuraiton confusions are prominent. The study reported by Mosher (61) demonstrates that training which emphasizes word form produces a better performance in an actual reading situation than with isolated words. However, analysis and the stressing of cues in word perception seems desirable for poor readers. Dearborn's analysis (26) indicated that both word form and context are influential in the perception of words by children. Smith (81) also concluded that in perceiving words children are influenced largely by certain characteristics of word wholes. Although Smith (81) and Gates and Boeker (35) found that the ends of words were most confused in word perception by children, Meek (57) and Wiley (109) report that beginnings of words are most confused.

These studies of word perception in children indicate that many factors are involved, none of which operates to the exclusion of others in any child. While total word form is important, probably it is not essential in many situations. Other prominent factors to be considered are dominant and characteristic letters or letter groups, word length, and various minute details. On the whole, total form

appears to be of less importance in word perception with children than with adults. This view is supported by the fact that, in adult reading, word form is most effective in reading familiar material. Obviously, words are relatively less familiar to children than to adults.

## BIBLIOGRAPHY


1. ABERNETHY, E. M., Photographic Records of Eye-movements in Studying Spelling. *J. Educ. Psychol.*, 1929, **20**, 695–701.
2. ACH, N., Über die Beeinflussung der Auffassungsfähigkeit durch einige Arzneimittel. *Psychol. Arb.* (Kraepelin's) 1900, **3**, 203–288.
3. ANDERSON, G. L., *La Lecture Silencieuse*, Delachaux and Niestle, S. A., Paris, 1929.
4. ANON., Movements of the Eyes in Reading. *Lancet*, 1928, **214**, 664.
5. ANON., Reading and Spelling. *Lancet*, 1928, **214**, 1132–1133.
6. BLACKHURST, J. H., *Investigations in the Hygiene of Reading.* Baltimore, Warwick and York, 1927.
7. BOGGS, L. P., How Children Learn to Read: An Experimental Study. *Ped. Sem.*, 1905, **12**, 496–502.
8. BOOK, W. F., Analysis of the Task of Learning to Read. *J. Educ. Res.*, 1930, **21**, 1–6.
9. BOOK, W. F., Various Methods of Mastering New Words While Learning. *J. Educ. Res.*, 1930, **21**, 81–94.
10. BOOK, W. F., The Development of Higher-Orders of Perceptual Habits in Reading. *J. Educ. Res.*, 1930, **21**, 161–176.
11. BOWDEN, J. H., Learning to Read. *Elem. School Teacher*, 1911, **12**, 21–33.
12. BROWN, A. F., The Relation of Heterogeneous and Homogeneous Chromatic Stimuli in the Range of Visual Apprehension Experiment. *Amer. J. Psychol.*, 1929, **41**, 577–594.
13. BROWNELL, W. A., The Development of Children's Number Ideas in the Primary Grades. *Sup. Educ. Monog.*, 1928, No. 35.
14. BURTT, H. E., BECK, H. C., and CAMBELL, E., Legibility of Backbone Titles. *J. Appl. Psychol.*, 1928, **12**, 217–227.
15. BUSWELL, G. T., *A Laboratory Study of the Reading of Modern Foreign Languages.* New York, Macmillan, 1927.
16. CHAO, Y. J. A., Comparison of Vertical and Horizontal Writing of the Chinese Characters. (Chinese) *New Educ.*, 1925, **10**, 663–680.
17. CHOU, S. K., A Quadrant Tachistoscope for Studying the Legibility of Chinese Characters. *J. Exp. Psychol.*, 1929, **12**, 178–186.
18. CHOU, S. K., Reaction-Keys and a New Technique for Reading-Reactions. *Amer. J. Psychol.*, 1929, **41**, 469–473.
19. CHOU, S. K., Reading and Legibility of Chinese Characters. *J. Exp. Psychol.*, 1929, **12**, 156–177.
20. CHOU, S. K., Gestalt in Reading Chinese Characters. *Psychol. Rev.*, 1930, **37**, 54–70.
21. CHOU, S. K., Tachistoscope: vs. Bradyscope. *Amer. J. Psychol.*, 1930, **42**, 303–306.

22. COWAN, A., Test Cards for Determination of Visual Acuity: A Review. *Arch. Ophth.*, 1928, **57**, 283–297.
23. COWAN, A., Test Letters which Comply with the Physiological Requirements of a Visual Test Object. *Amer. J. Ophth.*, 1928, **11**, 625–628.
24. COWAN, A., A Visual Test Card. *Amer. J. Ophth.*, 1929, **12**, 301–303.
25. CROSLAND, H. R., The Influence of Letter Position on the Range of Apprehension—A Reply to Dr. Tinker. *Psychol. Bull.*, 1929, **26**, 375–377.
26. DEARBORN, F. R., A Study of Erroneous Word Concepts in Reading. *El. Eng. Rev.*, 1929, **6**, 3-6; 23.
27. DEARBORN, W. F., *Special Reading Studies*, pp. 108–115 (Unpublished manuscript, cited by C. S. Payne, **68**).
28. DECROLY, O., and DEGAND, J., Expériences de mémoire visuelle verbale et de mémoire des images, chez des enfants normaux et anormaux. *L'Année Psychol.*, 1907, **13**, 122–132.
29. DE MARCHI, S., Le valutazioni numerichi di colletivita. *Arch. Ital. di psicol.*, 1929, **7**, 177–255.
30. DOLEZAL, J., Untersuchungen über die Verwendbarkeit 6stelliger Zahlen für Kraftwagen. *Psychotechn. Zsch.*, 1927, **2**, 148.
31. FERREE, C. E., and RAND, G., Intensity of Light and Speed of Vision Studied with Special Reference to Industrial Situations. Part I. *Trans. Illum. Eng. Soc.*, 1927, **22**, 79–110.
32. FERREE, C. E., and RAND, G., Intensity of Light and Speed of Vision Studied with Special Reference to Industrial Situations. Part II. *Trans. Illum. Eng. Soc.*, 1928, **23**, 507–546.
33. GATES, A. I., Functions of Flash-Card Exercises in Reading: An Experimental Study. *Teachers College Record*, 1925, **27**, 311–326.
34. GATES, A. I., *New Methods in Primary Reading*. New York, Teachers College, 1928, esp. pp. 10–23.
35. GATES, A. I., and BOEKER, E., A Study of Initial States in Reading by Pre-school Children. *Teachers College Record*, 1923, **24**, 469–488.
36. GEORGES, J. S., The Nature of Difficulties Encountered in Reading Mathematics. *Sch. Rev.*, 1929, **37**, 217–226.
37. GLANVILLE, A. D., and DALLENBACH, K. M., The Range of Attention. *Amer. J. Psychol.*, 1929, **41**, 207–236.
38. GRAY, W. S., Summary of Reading Investigations. (July 1, 1927, to June 30, 1928.) I. *Elem. School J.*, 1929, **29**, 443–458.
39. GRAY, W. S., Summary of Reading Investigations. (July 1, 1927, to June 30, 1928.) II. *Elem. School J.*, 1929, **29**, 496–510.
40. GRAY, W. S., Summary of Reading Investigations. (July 1, 1928, to June 30, 1929.) I. *Elem. School J.*, 1930, **30**, 450–466.
41. GRAY, W. S., Summary of Reading Investigations. (July 1, 1928, to June 30, 1929.) II. *Elem. School J.*, 1930, **30**, 496–508.
42. GRZEGORZEWSKA, M., Struktura psychiczna czytania wzrokowego i dotykowego. II. *Polskie arch. psychol.*, 1927, **1**, 107–121.
43. HEIGL, O., Untersuchungen über das Lesen und die Zahlauffassung sowie deren Beeinflussung durch Tee. *Arch. f. d. Ges. Psychol.*, 1928, **64**, 257–300.

44. HEIMANN, A., and THORNER, H., Experimentelle Untersuchungen zur Psychologie des Lesens, II. Das Lesen von sinnvollem Material. *Arch. f. d. ges Psychol.*, 1929, **71**, 165–184.
45. HOFFMANN, J., Tachistoskopische Leseleistungen bei Kindern. *Ber. ü. d. Kongr. f. exper. Psychol.*, 1928, **10**, 122–125.
46. HOLT, E. B., Eye-Movement and Central Anaesthesia, *Psychol. Rev. Monog. Suppl.* No. 17 (Harvard Stud. I). 1903, 3–45.
47. HOVDE, H. T., The Relative Effects of Size of Type, Leading and Context. *J. Appl. Psychol.*, 1929, **13**, 600–629.
48. HOVDE, H. T., The Relative Effects of Size of Type, Leading and Context, Part II. *J. Appl. Psychol.*, 1930, **14**, 63–73.
49. HOWELL, H. B., A Fundational Study in the Pedagogy of Arithmetic. New York. Macmillan, 1914.
50. JUDD, C. H., Psychological Analysis of the Fundamentals of Arithmetic. *Suppl. Educ. Monog.*, No. 32, 1927, esp. pp. 16–52.
51. KAO, P. S., and CHA, L. C., *An Observation of Eye Movements in Reading Chinese and English*, 1919. (Unpublished thesis, Univ. of Chicago.)
52. KUTZNER, O., Tachistoskopische Untersuchungen über das Lesen. *X Kongress f. exp. Psychol.*, Bonn, 1927.
53. LAY, W. A., *Führer durch den Rechenunterricht der Unterstufe.* Leipzig, Quelle and Meyer, 1907.
54. LEIRI, F., Über die Wahrnehmung Während Augenbewegungen. *Graefes Arch. f. Ophth.*, 1927, 119, 711–718.
55. LUCKIESH, M., *Light and Color in Advertising*, New York, D. Van Nostrand, 1923 (Legibility in Color Contrast) 246–251.
56. MARY, SISTER (of the Visitation), Visual Perception in Reading and Spelling; a Statistical Analysis. *Cath. Univ. Amer., Educ. Res. Bull.*, 1929, 4, No. 1.
57. MEEK, L. H., *A Study of Learning and Retention in Young Children.* New York, Teachers College, Columbia U. Contr. to Ed. No. 164. 1925 (Cues Used by Children in Learning to Recognize Words), 50–61.
58. MILES, W. R., The Peep-hole Method for Observing Eye Movements in Reading. *J. Gen. Psychol.*, 1928, **1**, 373–374.
59. MILES, W. R., and SEGEL, D., Clinical Observation on Eye Movements in the Rating of Reading Ability. *J. Educ. Psychol.*, 1929, **20**, 520–529.
60. MILES, W. R., and BELL, H. M., Eye-Movement Records in the Investigation of Study Habits. *J. Exp. Psychol.*, 1929, **12**, 450–458.
61. MOSHER, R. M., Some Results of Teaching Beginners by the Look-and-Say Method. *J. Educ. Psychol.*, 1928, **19**, 185–193.
62. NEWHALL, S. M., Instrument for Observing Ocular Movements. *Amer. J. Psychol.*, 1928, **40**, 628–629.
63. NEWLAND, T. E., A Study of the Specific Illegibilities Found in the Writing of Arabic Numerals. *J. Educ. Res.*, 1930, **21**, 177–185.
64. NIXON, H. K., A Study of Perception of Advertisements. *J. Appl. Psychol.*, 1927, **11**, 135–142.
65. PARSONS, J. H., A Review of Report of Committee Appointed to Select Best Faces of Type and Display for Government Printing and of

Note on Legibility of Printed Matter by L. A. Legros. *Brit. J. Ophthal.*, 1922, **6**, 475.

66. PATERSON, D. G., and TINKER, M. A., Studies of Typographical Factors Influencing Speed of Reading. II. Size of Type. *J. Appl. Psychol.*, 1929, **13**, 120–130.
67. PATERSON, D. G., and TINKER, M. A., Time-Limit vs. Work-Limit Methods. *Amer. J. Psychol.*, 1930, **42**, 101–104.
68. PAYNE, C. S., *The Derivation of Tentative Norms for Short Exposures in Reading.* Cambridge, Mass.: Harvard Univ. Press, 1929.
69. PICKFORD, R. W., *Studies in the Psychology of Reading: Three Experiments on the Relation of Meaning to Perception in Reading.* H. M. Stationery Office, London, 1929.
70. PILLSBURY, W. B., and MEADER, C. L., *The Psychology of Language.* D. Appleton & Co., 1928, pp. 129–151; 152–186.
71. POLLOCK, M. C., and PRESSEY, L. C., An Investigation of the Mechanical Habits in Reading of Good and Poor Readers. *Educ. Res. Bull.* (Ohio State Univ.), 1925, **4**, 273–275.
72. PONZO, M., Urteilstäuschungen über Mengen. *Arch. f. d. ges. Psychol.*, 1928, **65**, 129–162.
73. PONZO, M., Illusioni negli apprezzamenti di collettivita. *Arch. ital. di psicol.*, 1928, **7**, 1–37.
74. PRESSEY, S. L., and PRESSEY, L. C., *A Chart for the Diagnosis of Illegibilities in Handwriting.* Bloomington, Public School Publ. Co., no date.
75. PRESSEY, S. L., and PRESSEY, L. C., Analyses of Three Thousand Illegibilities in the Handwriting of Children and Adults. *Educ. Res. Bull.*, 1929, **6**, 270–273; 285.
76. PRESSEY, L. C., *Pressey's Diagnostic Tests in the Fundamental Reading Habits.* Bloomington, Public School Publ. Co., 1928.
77. PRESSEY, L. C., *A Manual of Reading Exercises for Freshmen.* Columbus, Ohio State University Press, 1928.
78. PYLE, W. H., The Reading Span. *Elem. School J.*, 1929, **29**, 597–603.
79. RAUSCHBURG, P., Ueber Hemmungs gleichzeitige Reizwinkungen. *Zsch. f. Psychol.*, 1902, **30**, 39–86.
80. SCOTT, W. D., *The Theory of Advertising.* Boston, Small, Maynard & Co., 1903 (Legibility of a Time Table), 119–129.
81. SMITH, N. B., Matching Ability as a Factor in First Grade Reading. *J. Educ. Psychol.*, 1928, **19**, 560–571.
82. STARCH, D., *Advertising,* New York, Scott, Foresman and Co., 1914, (Capital, Italic, Roman Type) 181–182; (Length of Line) 186–187; (Effect of Background) 189–190.
83. STARCH, D., *Principles of Advertising,* Chicago, A. W. Shaw Co., 1923, (Capital, Italic Roman Type) 657–658; (Length of Line) 663–668; (Effect of Background) 668–669.
84. STEIN, W., Tachistoskopische Untersuchungen über das Lesen mit sukzessiver Darbietung. *Arch. f. d. ges. Psychol.*, 1928, **64**, 301–346.
85. THORNER, H., Experimentelle Untersuchungen zur Psychologie des Lesens. I. Das Lesen von sinnleerem Material. *Arch. f. d. ges. Psychol.*, 1929, **71**, 127–164.

86. TINKER, M. A., Legibility and Eye Movement in Reading. *Psychol. Bull.*, 1927, **24**, 621–639.

87. TINKER, M. A., Visual Apprehension and Perception in Reading. *Psychol. Bull.*, 1929, **26**, 223–240.

88. TINKER, M. A., How Formulae are Read. *Amer. J. Psychol.*, 1928, **41**, 476–483.

89. TINKER, M. A., Numerals versus Words for Efficiency in Reading. *J. Appl. Psychol.*, 1928, **12**, 190–199.

90. TINKER, M. A., A Photographic Study of Eye Movements in Reading Formulae. *Genet. Psychol. Monog.*, 1928, **3**, No. 2, 68–182.

91. TINKER, M. A., The Relative Legibility of the Letters, the Digits, and of Certain Mathematical Signs. *J. Gen. Psychol.*, 1928, **1**, 472–496.

92. TINKER, M. A., Eye Movement Duration, Pause Duration, and Reading Time. *Psychol. Rev.*, 1928, **35**, 385–397.

93. TINKER, M. A., The Influence of Letter Position on Range of Visual Apprehension—A Reply to Dr. Crosland. *Psychol. Bull.*, 1929, **26**, 611–613.

94. TINKER, M. A., Photographic Measures of Reading Ability. *J. Educ. Psychol.*, 1929, **20**, 184–191.

95. TINKER, M. A., and PATERSON, D. G., Influence of Type Form on Speed of Reading. *J. Appl. Psychol.*, 1928, **12**, 359–368.

96. TINKER, M. A., and PATERSON, D. G., Studies of Typographical Factors Influencing Speed of Reading. III. Length of Line. *J. Appl. Psychol.*, 1929, **13**, 205–219.

97. TINKER, M. A., ROBERS, D., and JACKSON, H., Definite and Indefinite Preparation in the Visual Apprehension Experiment. *Amer. J. Psychol.*, 1930, **42**, 96–100.

98. TSAI, L. S., and ABERNETHY, E., The Psychology of Chinese Characters, *J. Exp. Psychol.*, 1928, **11**, 430–442.

99. TSANG, Y. Y., *An Experimental Study of the Effect of Vertical and Horizontal Alignment and of the Use of Punctuation Marks upon the Rate of Reading the Chinese Language.* (Unpublished Thesis, Univ. of Washington.) 1926.

100. TU, H. T. C., *A Comparison of Rate and Comprehension of Silent Reading Between Two Languages.* (Unpublished thesis, Univ. of Iowa.) 1923.

101. TU, H. T. S., The Effect of Different Arrangements of the Chinese Language Upon Speed and Comprehension of Silent Reading. *Chinese Soc. and Pol. Sci. Rev.*, 1926, **10**, 278–301.

102. TU, H. T. C., Horizontal and Vertical Arrangement of Chinese Characters. (*Chinese*) *Educ. Rev.*, 1926, **18** (No. 11).

103. VERNON, M. D., A Method for Observing the Position and Direction of Movement of the Eye. *J. Gen. Psychol.*, 1929, **2**, 147–149.

104. VERNON, M. D., Methods of Recording Eye Movements. *Brit. J. Ophth.*, 1928, **12**, 113–130.

105. VERNON, M. D., The Movements of the Eyes in Reading. *Brit. J. Ophth.*, 1928, **12**, 130–139.

106. VERNON, M. D., *Studies in the Psychology of Reading: The Errors Made in Reading.* H. M. Stationery Office, London, 1929.

107. VERNON, M. D., The Relationship of Subjective Experience to the Performance of Eye Movements. *Brit. J. Psychol.*, 1929, **20**, 161–172.

108. WARTENSLEBEN, G., Ueber den Einfluss der Zwischenzeit auf die Reproduction gelesener Buchstaben, *Zsch. f. Psychol.*, 1913, **64**, 321–385.

109. WILEY, W. E., Difficult Words and the Beginner. *J. Educ. Res.*, 1928, **17**, 278–289.

110. YOURIÉVITCH, M. S., Physiologie—Une nouvelle méthode d'enregistrément des mouvements oculaires. Le cinegraphe oculaire. *Comptes rendus des séances de l'Académie des Sciences*, 1928, **187**, p. 844.

# VISUALLY DETERMINED REACTIONS IN THE VERTEBRATES[1]

BY ROSS STAGNER
*University of Wisconsin*

The desire to place human vision in its proper historical setting, and to increase our knowledge of the controls of human behavior, has led to an extraordinary amount of work in physiological and psychological laboratories on the visual function in animals. Of this the major portion, of course, has been concerned with vertebrates. The unique structure of the invertebrate eye, and the presence of other sense organs corresponding to nothing in the human species, prevent us from drawing analogies as to invertebrate visual functions. This handicap is in part avoided when we study the phylogenetic group to which we ourselves belong. It is further true that vertebrates make up most of the significant fauna of our environment; insects and their ilk are usually looked upon as something to be rid of, rather than to be used to our advantage. Such reasons may be adduced to explain the amount of energy which has been expended upon laboratory and observational studies of visual control of behavior in the vertebrates.

At the lower end of the vertebrate series we are obliged to study sensory capacity in terms of the tropism. The basis of a tropistic response, as it is now fairly widely accepted, consists of an asymmetrical stimulation of a receptor, resulting in an immediate raising or lowering of the muscle tensions in a definite way with respect to the source of the stimulation. It follows that tropistic responses are not likely to be differential. A stimulus either disturbs the physiological equilibrium, or it does not. The transition from tropistic to so-called "higher" forms of behavior is marked by the appearance of differential responses. That this transition occurs in individual as well as species development is easily understandable to anyone who has ever worked with very young babies. The positive phototropism is only one of several characteristic responses which may be observed. Later this condition undergoes gradation and

[1] Presented to Dr. Henmon's Seminar in Psychology, April 17, 1930.

differentiation, representing what we describe in the field of reflexes as negative adaptation. The modifiability of behavior in general, which characteristically increases as we ascend the phylogenetic scale, may probably be described with reasonable adequacy as an increase in the susceptibility of tropistic responses to negative adaptation.

The essential similarity between the tropism and the configuration, as expounded by the Gestalt school, will readily be apparent. The primary characteristic of the Gestalt, as an explanatory principle in animal behavior, is that it represents a strictly deterministic reaction of the animal to asymmetrical stimulation. Orientation takes place in terms of a differential influence from the environment, whether expressing itself as variations of muscle tonus or in the central nervous system. Whether this view is adequate for forms of behavior other than those of a strictly perceptual nature is not a matter of concern in this paper; whether it is adequate even for purely perceptual processes is open to question, and with this matter we shall be concerned in a later section.

Because the literature on visual reactions in the vertebrates is rather voluminous and deals with an extremely haphazard range of subjects, the problem of organizing it for a review has been critical. An attempt has been made to group the studies into four classifications, according to the portion of the vertebrate phylum being investigated: primates; lower mammals; birds; and fish, including the reptiles and amphibia. Under each of these headings the studies were grouped according to the particular function being studied, viz.: ability to react to light; color vision; brightness discrimination; size, form and pattern; and studies on Gestalt phenomena. Of these, color vision has attracted by far the most investigators, and, strangely enough, the fish have attracted most of the attention in this field. That this is probably due to the quarrel, of ancient standing, between von Hess and von Frisch as to the color vision of animals in general, which has provoked an undue amount of study in German circles, is obvious to a student of the literature.

The methodological question is also one of importance in studies of this sort. What criterion may legitimately set up as proof that the animal is influenced by the stimulus? In these studies four different types of response may be found: (1) a regular reaction whenever the stimulus is presented (tropistic responses, respiration changes, the psychogalvanic reflex); (2) the natural differential response, involving a multiple choice situation in which the indi-

vidual may express a preference; (3) the method of acquired response, as exemplified by the conditioned reflex method; and (4) the method of acquired differential response, in which the animal is presented with an asymmetrical situation to which he must react. The last includes the Yerkes apparatus and its variants, which include essentially a uniform stimulating situation which is asymmetrical with respect to a single detail.

As criteria in the interpretation of responses, we may legitimately set up the following: (1) avoidance of anthropomorphic inferences; (2) exclusion of extraneous cues from the experimental situation; (3) unquestioned demonstration of discrimination by a long enough series of responses with a statistically valid accuracy; (4) negative confirmation (conditions under which the response is lost); and (5) the law of parsimony—the simplest explanation of any behavior has preference. That it is not always easy to decide what is the simplest explanation does not improve matters any. The exclusion of the experimenter from the situation and the necessity for giving complete data and statistical treatment are American contributions to comparative psychology which are just beginning to make themselves felt in foreign countries.

The present review will probably be found to lack certain items intentionally and others inadvertently. We have attempted to exclude purely physiological studies of sense-organ function, and for the most part observational studies have been left out. The question of the relation of anatomical structure to manner of functioning, although we consider it important, has largely been passed over. On the other hand, several behavior studies may have been overlooked. Two or three have come to light in the few days since this paper was thought to be roughly completed. This review may not, therefore, deserve the title "complete," but it may pretend to the adjective "comprehensive," since it includes what are unquestionably the most important experimental studies that have ever been made on the visual function in vertebrates.

## A. Amphioxus, Fish, Reptiles, Amphibia

### *1. Integumentary Sensitivity*

The primitive vertebrate, Amphioxus, has no eyes in the ordinary sense of the word. Parker (89) has shown that the pigmented spot on the anterior end of the body is not concerned with reactions to light stimulation. Amphioxus is negatively phototropic,

and is only slightly more sensitive to light at the anterior than at the posterior end, as shown by cutting the animals in two and subjecting each separately to photic stimulation, [Nagel (85), Krause (75) and Parker (89)].

Pearse (93) found that eyeless amphibian larvae responded to all his monochromatic lights, but not by any differential response. Laurens (81) states that eyeless salamander larvae are about as strongly photopositive as normal individuals. Greene and Laurens (38) found that single ocular extirpation caused a slight disturbance in equilibrium in Amblystoma, but removal of the other eye restored the balance. Jorden (62) reports that the Hamlet, a marine fish, is still sensitive to light after ocular extirpation, and Van Heusen (134) found like conditions in the catfish. Payne (91) states that blind cave fish seem to show discomfort in light, and gather in the shade by random movement, not by any orientation with reference to the source of the light. Parker (90) sewed together the eyelids of the dogfish and found that positive phototropism remains, but objects are not avoided. This procedure does not insure blindness, as anyone can prove by closing his eyes and looking at a bright light. He also found (87, 88) that frogs whose optic tracts had been severed and headless larval lampreys orient toward the light, but do not necessarily move toward it. This shows at a glance the relative value of the eye and the skin as media of photic reactions. Bernard (5) suggests that skin sensitivity is not a fundamental state, but appears in the evolution of certain species in which it will be of advantage. He looks upon Amphioxus, not as a primitive, but as a degenerate vertebrate. While these hypotheses may have fitted well the facts known at that time, later discoveries enforce a different interpretation.

### *2. Ocular Reactions to Photic Stimulation*

Froloff (30a) conditioned the flashing of a light to an electric shock defense reflex in the fish. The light being flashed for 5″–15″ before application of the shock, he needed only 5 to 30 repetitions to set up a satisfactory conditioned response. Reese (98) found that the crimson spotted newt is photonegative. In a study on several varieties of snakes and on the Mississippi alligator (100, 99) he found negative phototropism and very little practical use of vision.

Riley (110) found that young toads react negatively to intense artificial light, positively to diffuse daylight and weak (16 c. p.)

artificial light, positive to blue and as often positively as negatively to red. Torelle (**132**) found that frogs normally orient toward the light, but when the temperature is below 10° C., the phototaxis is negative. Both of these sets of facts obviously have a close relation to the actual mode of existence of the frog.

Yerkes (**154**) studied the relation of photic stimulation to response aroused through another modality. By means of an ingenious hook-up to a Hipp chronoscope, he measured the reaction time of the frog to an electric shock, and observed that practically simultaneous stimulation gave facilitation, whereas an interval of two-three seconds resulted in almost total inhibition of the defense response. Hinsche (**58**) reports that the frog shows some ability to profit by optic stimulation in a problem situation. Kohlrausch and Schilf (**73**) found that even after application of curare the frog showed the galvanic skin reflex to optical stimuli. Ultra-violet rays were not effective. Schilf and Schuberth (**119**) extended this to decerebration and showed by various extirpations that the essential path for this response included the cord and the sympathetic.

### *3. Color Vision*

Krause (**75**) attempted to test the color preferences of Amphioxus by illuminating a trough containing a large number with a spectrum, and counting after a certain period, the number in each band. He found no evidence for selective sensitivity. Hess (**50**) observed the activity of Amphioxus under different spectral illuminations and found the greatest amount under yellow and green, from which he concluded color blindness. Since that is the only conclusion Hess ever reached, we begin to wonder if a Freudian factor of wish-fulfillment is involved.

Bauer (**3**) put fish in a trough blackened on the sides and presented colors at the end by cards or filters. He thought he found evidence for preferences and aversions. Under the haphazard conditions of his experiment, he might have found evidence for almost anything. Dunn (**22**) noted the color changes of fish to conform to their background, and made the assumption, now generally disapproved of, that these color changes indicated color sensitivity.

Hess (**49, 51, 52, 53, 54**) has presented arguments in abundance and some experimental evidence against the existence of color vision in fish. He placed fish over white, red and black backgrounds. The red fish (i.e., that one exposed to red) could not be distinguished from the black at the end of the time exposed. Hence he deduces

a brightness spectrum corresponding to color blind man's, and therefore color blindness. He argues further that the long wave lengths would be absorbed by the water anyway. Considering the question of an apparent instinctive fear of red, he claims that he found values for blue and red at which their chromotropic values were equal. He also tried some experiments on feeding colored larvae, but used such a technique that it would have been practically a miracle if he had gotten results for color discrimination. His work has been criticized by Reeves (102). She points out that his result on the chromotropic value of red and blue has not been found by Bauer; that he used young fish, in which the retinae might be incompletely developed; that he did not set up an avoiding response in his feeding experiments, and hence the fish might be expected to generalize from being fed with colored objects that all colored objects were food. She also shows that his experimental conditions were not controlled to prevent disturbance of the fish, and that his failure to get a response to red might have been an avoiding response to that color, which several investigators report.

The opposite side of the question has been presented with great regularity by von Frisch and his pupils. Frisch showed (27, 28, 29, 30) that the color changes involved in adaptation to a colored surrounding are dependent upon the eye and the central nervous system. He used this method of color adaptation to prove color vision in several species of fish. It is to be doubted if a satisfactory criterion of color vision could be set up on this basis. The change may require color vision in the sense that the retina is sensitive to differences in wave length, and yet the reaction be purely passive as far as the organism itself is concerned. We prefer to maintain that the ability to use colors as signals, in the sense that Pavlov has laid down for signaling reflexes, is essential to true color vision. It is certain that von Frisch's results by the color adaptation method were erratic and inconsistent, and he regularly has to explain away some color change as due to " psychic excitation " due to disturbance of the aquarium, etc. Only one of his experiments appears to be a really good test for color vision: he fed his fish from colored tubes, and then presented the same tubes in a series of grays ranging from white to black. He got evidence for good discrimination here. This method is the basis of most of the good work on color vision on animals—to have them pick out a given color by positive training, from a whole series of grays. If the discrimination de-

pends on brightness, there will be somewhere in the series a gray which will approximately match the color.

Freytag (**26**) tried 100 fish in experiments on color adaptation, along the lines used by von Frisch, and found no regular changes in color due to exposure to a given stimulus. Often the change was just the opposite of what he had expected. He argues that brightness differences would be sufficient to explain the consistencies which appear. Sumner (126) agrees with von Frisch that fish can change color and pattern to adapt to a ground, but states that they cannot choose a ground favorable to the existing pattern of their integument. The opposite is asserted by Mast (**83**) in his experiments on flounders. He would adapt the fish for six weeks to a given color, e.g., blue. He then presented the fish with a choice between blue and yellow, and got 88 per cent choices of blue. But when red was used as the adapted color, only 3 per cent (5 of 150 fish) chose red. This is too low to occur by chance, and he interprets it as a strong instinctive fear of red. There may have been other factors involved, and he does not report any tests on brightness choices.

Polimanti (**94**) studied the effect of monochromatic light upon breathing responses in several fish. He got a pronounced increase in the red end for two species, variability on other colors. He concludes in favor of color blindness.

The most satisfactory methods of proving color vision involve feeding in connection with color training. These experiments have been going along for many years. Zolonitsky (**161a**) fed minnows with red bait, then tested them with bits of differently colored yarn. More jumped for the red yarns than for any other color. Goldsmith (**34**) tried feeding with differently colored forceps, and got some differential response. White (**148**) trained minnows and sticklebacks to leap from water for food when different colored papers were shown. The minnows learned to distinguish red from green and blue from yellow, but the sticklebacks were not capable of the second discrimination. Reighard (**103a**) fed a group of gray snappers living under a dock with minnows dyed brightly and found that there was no native response to bright colors. He then sewed a small nettle cell into the mouth of each red minnow. Within a few trials the red minnows were practically all rejected (there were about 150 fish in the school, hence some would not have had as much opportunity as others to learn the reaction). After three weeks the fish still rejected red, blue and yellow bait. Wash-

burn and Bentley (143) using red and green forceps, got the creek chub to snap at the proper colored forceps even when they were empty. In this study and that of Reighard's, several different brightnesses of each color were used—the earlier studies did not attempt to control this factor or, in many cases, that of smell as well. It is to be doubted that any method other than that of requiring the animal to select a given color from a series of grays ranging from black to white adequately controls the brightness factor.

A study of this kind is presented by Burkamp (13) when he used 24 food containers, two of the positive color and the others of different colors. In the test series seventeen grays of varying brightness and seven colored boxes were used. The fish recognized the color under these conditions, when it was illuminated by monochromatic light of its own wave length, and by light of a complementary color. By an analysis of the errors made, he concluded that chroma, saturation and brightness were, in order of importance, the factors influencing the choices. Schaller (117) in an experiment not so well controlled, used discs of colored paper on needles. Just below the disc was the bit of food. His minnows learned to discriminate colors by this method.

Experiments using spectral illumination offer better ground for accepting evidence of color vision, although, as we shall show later, the criticisms of colored papers in this type of work are not entirely valid. Reeves (102) was the first to adapt the Yerkes type of problem box to the use of fish. She used spectral illumination with dace and sunfish and found that they could make a discrimination and hold it in spite of changes in the relative intensity of the two lights. Check tests on discrimination of intensities showed that differences in intensity of less than 100 per cent were not discriminated in one case and of less than 300 per cent in another. She also counted the respiration under diffuse light and found it to be 60 per minute, under bright light 85, but under a relatively dim red spectral light it increased to 150.

Schiemenz (118) used colored papers on different ends of the aquarium and shocked the fish for approaching that end. They learned to discriminate a color from a series of grays, and from other similar hues, although the more similar the hue, the greater the number of errors made. Spectral illumination in a control series gave the same results. Hamburger (46) trained eight minnows to jump for food on a stick held in a certain area of the

spectrum. They never reached perfection, but got above 90 per cent correct choices. He trained them to choose white against nine colors, and then tested for the phenomenon of complementary colors by mixing two which gave a gray for the human eye. He found that 47 per cent of the choices were correct, which is much higher than chance. Orange, which was most confused, received 26 per cent choices. Wolff (**152**) fed minnows in a particular area of the spectrum and found some individual differences in mode of reaction. Some minnows seemed to be color-blind, in that they persistently reacted to brightness value. He found that minnows as a rule can see at the red end of the spectrum at least as far as human beings, and at the violet end to a wave length of 340. He was able to train with fair accuracy to twenty different color nuances. He mentions that the color circle is closed for the minnow, as evinced by the fact that violet and red hues are confused at times. Hineline (**57**) used Wratten gelatine filters to get monochromatic light, and found his minnows able to discriminate red from green, red from blue, red from yellow and yellow from blue, but not blue from green. He controlled the energy values of the lights, but no other brightness factor. He did, however, exclude the infra-red and ultra-violet rays.

Color vision in the reptiles and amphibia has not been the scene of much experimentation. Babak (**3**) using the respiration method, found a higher stimulating value for violet light in small quantities. Red was very weak. Pearse (**93**) illuminated amphibian larvae with different colored lights. He found violet most and red least effective.

We may conclude that within the limits of individual differences fish are susceptible to differences in wave length of light, but for other members of this group the evidence is too scanty.

### *4. Form and Pattern*

Schiemenz (**118**) using the method of electric shock, got a differentiation of forms. Schaller (**117**) feeding with needles, as described above, got discrimination of circle, star, rectangle and triangle. An emphasis on color or form in the original experiments caused that factor to determine the reaction in experiments in which color or form alone were presented.

Casteel (**15**) trained two turtles to discriminate a horizontal from a vertical bar of the same size. These two learned to discriminate parallel lines eight mm. in width from lines two mm. in

width. One turtle was able to discriminate a difference of one mm. in width.

Schaeffer (116) found that feeding hairy caterpillars to frogs quickly set up a discrimination against them. Four to seven trials were sufficient. Judging from the caterpillars which were accepted, he concluded that form and brightness were determining factors.

Yerkes (155) put tortoises of different habitats on a board over an aquarium. Water-living species jump at any height, although unwillingly from 180 cm. The water-land turtles were slower at jumping, and the land turtles would not jump at 180 cm. If blindfolded, most species jumped, indicating a visual response of some kind.

Buijtendijk and Remmers (12) fed fish on white bread and white chalk and got an irregular curve which never reached the base-line in learning. If black bread or black chalk were used the learning was accelerated.

### *5. Gestalt*

Gestalt experiments go back to the time before Wertheimer started publishing his views on the subject of perception. Bauer (4) in 1909 was testing the phototropism of fish. He placed them in a blackened trough with light at one end. On increasing the illumination, he observed that the fish were photopositive, i.e., they swim toward the light. After a time with this illumination they distribute themselves throughout the aquarium, having reached an equilibrium of some sort. On dimming the light he was surprised to see that the fish swim *toward* the blackened end of the trough. He interpreted this as an after-image phenomenon, but we should be more inclined to say that a reduced illumination on one side would give the same result as increase of illumination on the other—that is, there is a response in terms of the difference in stimulation.

## B. Mammals

### *1. Reaction to Light*

Hahn (45), in studying the sensory adaptations of the bat, found that they were sensitive to light, but avoided objects as well after ocular extirpation as before. Hara (47) found the psychogalvanic reflex in curarised cats, but not in dogs, to optical stimuli. The deficiency of sweat glands in the dog may explain this.

### 2. Color Vision

Allen (1) found no evidence favoring color vision in the guinea pig. Apparently she used the Yerkes apparatus, although this is not clear from her report. Davis (20) studied the behavior of raccoons by feeding them in colored boxes. When he tested them with grays he found that they were responding to brightness differences. In spite of this, Cole (17), using his card displayer, in which the coons were supposed to react positively to one series of colors, and negatively to another, concluded in favor of color vision without a check of brightness. Gregg and McPheeters (37) found that the raccoons were able to touch the levers with their noses and may have gained a cue that way. Cole used no brightness control, but Gregg, Jamison, Wilkie and Radinsky (36) inserted one. They were using three pairs of colored discs, one combination meaning food, the other "no food." Substituting grays, they found that the coon made as many correct choices as before. Cole and Young (18) used the food-glass method with papers apparently equated for brightness. Smell was controlled and in control series grays were poorly discriminated. Hence we find it hard to say that brightness is the sole factor involved.

Colvin and Burford (19) painted boxes in nine different colors for their dogs, cat and squirrel. The animal was trained positively to one color by successive pairs and then required to pick it out from nine. Both brightness and cues from the experimenter are uncontrolled factors in this experiment. Sackett's work (114) on the porcupine is open to similar criticisms.

Yerkes (156) led the way in developing reliable methods for studying visual factors. He found that some of his dancing mice may have been relying on chroma, although the evidence is not definite. The red end of the spectrum is very weak. Waugh (147) trained his mice to distinguish Bradley orange-red and blue papers, but they could not discriminate when filters were used. Albinos were not as good at this as pigmented specimens. Washburn and Abbott (142) used a box in which the rabbit had to push a swinging door aside with his head to get food. On the door was pinned a colored or gray paper. This probably is more effective as a training method than the typical Yerkes method. The rabbit could not discriminate the red from a very dark gray, number 46 in the Hering series. DeVoss and Ganson (21) found that their cats

could discriminate colored papers from gray papers, but gray cambric confused them. Red, blue and violet are low in stimulating value for them, which is surprising in view of the nocturnal habits of the cat. Kittredge (70), using the Washburn method, found that the calf discriminated red from Hering gray number 5 but not from number 15.

The use of spectral illumination in this field is also very desirable. Watson and Watson (146) employed monochromatic light in training and found that the reactions disappeared upon varying the intensity. Long waves have a low stimulating value for them also. Hopkins (59) used the Yerkes apparatus with slight modifications, training mice to colored papers. Those trained to blue could not distinguish it from Hering gray number 13. Red was confused with number 49. Only one mouse was not confused by any gray. He criticizes the criticizers of colored papers by pointing out that mice trained on colored papers transferred without disturbance to the use of spectral illumination presented in a similar patch. He also objects to the usual " duplicity theory " of color vision as based upon the difference between rods and cones. Cones were lacking in the one mouse which apparently differentiated all colors successfully, and mice lacking rods were able successfully to set up the brightness response. Extirpation was used to prove that visual cues were actually being used.

The color vision of the dog has been the subject of much discussion and special study, although, strangely enough, there is a general consensus of opinion that he is color-blind. Samojleff and Pheophilatkowa (115) used the food box method and checked responses against a series of grays. At least one gray gave difficulty with every color. Smith (122) used a four-choice box with colored 10 x 12-inch cards. For each color some gray was found which caused confusion, though not necessarily complete disintegration of the color response. Orbeli in Pavlov's laboratory (see 160) failed to set up a conditioned reflex to color in several hundred trials. Since this is the most delicate method which has probably ever been devised for studying differential sensitivity, it is not likely that the dog has color vision. Razran and Warden (97) point out that it is reported that Walker, in Bekhterev's laboratory, set up a response to color after three thousand repetitions, but they were unable to check this.

Stratton (124, 125), for the sake of destroying another popular illusion, took some red banners and waved them in front of some

wild California cattle (it is to be noted that he did not hold the banners himself) and found that red did not attract as much attention as white. Blood on cloth or in a pail caused no excitement.

### *3. Brightness*

Yerkes (156) verified Weber's law with respect to discrimination of illumination by the dancer. He found a discrimination of shades of gray. He reported later (157) that age and sex did not modify ability to form this discrimination. Szymanski (128) found that rats could not discriminate a 10 candle-power light from darkness with 109 trials. This is hardly enough time for a good discrimination to be set up. Vincent (137) painted the true path of a maze white and the alleys black and found that it facilitated learning and speed of run. There was a noticeable transfer to brightness discrimination—animals from the normal maze took 66.6 trials to set up a brightness discrimination, while the black-white maze-trained rats took only 34.6. Pearce (92) trained rats to choose a lighted passage in studying habit interference. Bills and Maukin (6) trained rats to their limen in brightness discrimination and then subjected them to methyl alcohol fumes for 11–24 days. The rats maintained the limen. There was no test of ability to attain a limen while subjected to the fumes. Warden and Aylesworth (139) report that reward plus punishment is the best motivation for learning this habit, punishment being second and reward alone third. Hubbard (60) has made an analytical study of the brightness habit. She used the original Yerkes type apparatus, in which the stimulus patches are against the end wall, and another type in which the rat crawled through the illuminated circle to get food. The second was learned much more quickly. She suggests several explanations: (a) An increased brightness as a result of one less glass thickness in the latter type; (b) tactual and kinesthetic reinforcement; (c) frequency (the rat need not see the circle in the Yerkes type, every time he makes a choice); (d) configuration, the hole to be crawled through being a simpler configuration that that of a circle to be looked at. Of these, (b) and (c) have the best chance of being correct. Fields, whose work we shall discuss in a few moments, found that his blind rats continued to make correct choices as long as allowed to crawl through the holes in the form board.

Brightness discrimination in the dog was found by Stone to be very accurate. He reports (123) that differences as small as 0.14 c. p. gave thirty errorless trials in his female dog, and 0.2 c. p. for

the male. This was against a standard of 1 c. p. Frolow (see **160**) found a threshold for light intensity of 0.02. He could distinguish number 49 from number 50 in a series from 0.1 to 0.5 candle meters. Frolow and Orbeli both report conditioning to changes in illumination of the room, but do not give any figures.

Munn (**84**) made a careful study of discrimination in the raccoon and found that pattern differences were seen only as brightness differences. The coon could discriminate differences of 33 per cent in brightness.

Szymanski (**129**) could not get a brightness discrimination from his dogs until one alley was completely dark. Sutherland (**127**) found that his dogs could not discriminate one light from two lights, or even from four lights. Williams (**149**) obtained a discrimination of a flickering from a steady light.

### *4. Form and Pattern*

Lashley (**79**) taught one rat to discriminate a vertical from a horizontal bar. Rectangles of varying width and circles of 30–50 mm. diameter were discriminated, but a circle could not be distinguished from a square of equal area and brightness. Reeves (**101**) found that moving lights were more effective than still ones for the visual discrimination habit. Fields (**24, 25**) found that rats discriminated circle from six-pointed star, triangle with apex up from one with apex down, and the same above a circle, also a large circle with a smaller one on each side from one with smaller ones above and below. Area of forms was the same. Odor cues were controlled. Munn points out that the forms from the point of view of the rat's eye were unequally illuminated on account of the fact that in Fields' apparatus the forms were to be crawled through and the illumination came from a glass partition set several inches behind the form-board itself. Tactile cues are also indicated from the fact that blinded rats made correct choices while crawling through the actual forms.

Carr and Watson (**14**) state that blinded rats oriented as quickly in the midst of a well-known maze as did normal individuals. Richardson (**108**) made rats jump from one platform to another. Vision served for orientation, but if the distance were suddenly made shorter, the rat would overshoot his mark, indicating poor visual control. Higginson (**56**) put a snug-fitting door at the entrance to alley 3 in the circular maze used by Watson. Rats running the maze were made to run down the cul-de-sac, then allowed to con-

tinue to the food box. After 100 trials the door was left open, and four of the nine rats immediately took the shorter path. The other five soon changed. Valentine (135) repeated this experiment and found that of nineteen rats, only four took the new shorter path on the 101st trial, and only two on the second. Eighty trials were necessary to eliminate this blind alley completely. Johnson (63, 65) found that the dog is incapable of distinguishing a striated field from a plain one, and is incapable of reacting to angles. Shenger-Krestovnikova (see 160) got discrimination of one circle from another twice its size, of ellipses of axis ratio up to 4:5. Goubergritz is said to have established the 8:9 ratio, but the authors were unable to report definitely. Orbeli got discriminations of the cross from other figures, which persisted in spite of changes in size relations. Shenger-Krestovnikova, Orbeli and Wurtzel report vertical distinguished from horizontal movement, and Shenger-Krestovnikova and Goubergritz report clockwise from anti-clockwise rotation. Williams (149) got no form discrimination from his dogs.

### *5. Gestalt*

Hadley (42) trained guinea pigs to react to a square, A, which was larger and brighter than square B. Then insert instead of A the square C, which is not as large and bright as B, and the guinea pigs choose B. The animals were also trained on a single variable, size. Gayton (33) trained rats to choose B against A, then transferred to B against C. C does not have as many correct choices as B had in the training series, hence there may have been some absolute discrimination of B. If A-C were used, C had a higher percentage of correct choices, probably due to greater difference. He used the amount of illumination in the alleys as stimuli.

## C. Birds

### *1. Color Vision*

Porter (95) initiated the study of color vision in birds. Using the English sparrow and (96) the cowbird, he got discrimination of colors but not of forms, placing colored papers around feeding glasses. He did not check for brightness discrimination. Rouse (113) took respiration records for the domestic pigeon under different monochromatic illuminations. Some preference was shown for the short-wave end of the spectrum. A food-glass discrimination was very poor, but better than form discrimination.

Katz and Revesz (66) in a rather clever but hasty study of vision in the hen, glued colored grains to cardboard and strewed grains of the positive color in among them. A perfect trial consisted of getting the ten positively colored grains in ten successive pecks. Hens learned four shades of red, blue, green and gray. These results were only for one hen. Studying color fatigue, they trained a hen to choose white in preference to red number three. After being subjected to strong red light for two hours, the hen chose the red in preference to white. Tests with other colors gave inconsistent results, and a retest might not coincide with either.

Breed (10) found that blue was of high intensity for the eye of the chick. In his experiments he set up a red-blue discrimination with blue positive, and the training seemed to carry over to a blue-white choice. The brightness cue is still possible, and too few tests were made to be conclusive. Thorndike (130) fed chicks on colored cards. The number of times a given card was pecked at was assumed to measure the preference for that color. It probably was only a question of the relative visibility of the grain on a given background.

The influence of dark-adaptation has been a frequent subject of investigation. Katz and Revesz (67) report the Purkinje phenomenon in owls. Maximum brightness was located in the yellow-green, corresponding to the color-blind human eye. Honigmann (163) found that red light is much brighter for the light-adapted eye of the chick than for the human eye. At a wave length of 620 he calculated the brightness to be twice that for the human eye, at 640, three times and at 660, four times.

Hahn (44) had hens feed from grain illuminated by a spectrum. Her subjects fed all the way into the blue, in contrast to those of Hess, who is said to have found that hens stopped at the blue-green. Using the training method, she found that hens apparently had an "instinctive fear" of blue, but could overcome this.

Erhard (23) used the pupilloscope, a device with which one finds an intensity of white just matching a given colored light for dilating or contracting the pupil. The match is found when shifting from one light to another causes no change in the diameter of the pupil under observation. He found the birds' spectrum to be shortened more at the violet end than at the red. Probably day-birds are relatively blind to blue, night-birds to red. Yerkes and Eisenberg (159) set up a red-green discrimination, but a change in

the relative brightness of the colors broke it up. The Purkinje phenomenon is also reported in their doves (only two were used).

Watson (**145**) has studied the visible spectrum in chick and pigeon. The apex or point of maximum brightness seems to coincide with that for the human eye, at a wave-length of 5200 millimicra (.00052 mm.), but the limits of visibility for the chick were 3950 and 7150, for the pigeon 4200 and 7120. Blaesser (**9**) finds that there are considerable individual differences in color sensitivity in hens. Dark adaptation reduces the sensitivity to short waves; toward long waves the effect varies, some hens being more sensitive to red and others less. He finds what he calls a relative partial blindness to yellow, blue and green.

Katz and Revesz (**68**) found that training hens not to peck at grains of a certain color did not influence their behavior toward grains illuminated by light of that color. Reichner (**103**) studied complementary effects in hens. The positive color was complementary to the negative color used. Two hens were trained to feed in red light but not in green, while two were trained to feed in green. When the discrimination was well established, the positive light was suddenly weakened during a feeding. He found that in 19 of 21 trials the hen stopped feeding (i.e., the complementary color was sensed, presumably). On check experiments, if the weaker light were presented first, the color was reacted to as positive, indicating that brightness was not the cue. The dimming of the light may have been the real cue reacted to. Another contrast experiment is reported by Revesz (**104**) who trained hens to feed only from a green card. A green square would be presented with other colors (varied from time to time) surrounding it. Several shades of green were used. He now presented a gray square surrounded by red and the hen fed, but a gray surrounded by other colors was untouched. Evidently the phenomenon of simultaneous contrast is present here.

Kroh and Scholl (**77**) set up a response to a blue circle as opposed to a red triangle. On presenting a red circle and a blue triangle they found that the birds reacted to color, but could be trained to choose form. They suggest the term "reaction to partial content" instead of the more subjective "abstraction" to describe such phenomena.

Lashley (**80**) was able to set up a differential response to a wave-length of 6500 millimicra as against 5200, and to 5880 as

against 5000, in his chicks, and these persisted in spite of changes in relative energy value of the two lights.

Wood (153) reports an observational study on the satin bower bird. It may be taken as typical of nineteenth century misinterpretations and unjustified explanations of animal behavior.

### 2. *Brightness*

Tugman (133) reports studies on the brightness threshold of the sparrow. A Yerkes type box was used with standard lamp of .098 Hefners. The three sparrows used could discriminate differences of 0.015, 0.122 and 0.035 Hefners, respectively. The threshold for three humans was 0.009 to 0.013.

### 4. *Form and Pattern*

Porter (95) trained his sparrows to pick a glass marked with three horizontal bars from that marked by a diamond. His birds could not learn to choose on the basis of shape of the food glass.

Rouse (113) set up a discrimination for form in his pigeons, but it was very poor. Breed (10) got a size discrimination in his chicks, but no form discrimination. Bingham (7) found a size discrimination of 1 to 1.5 cm. on a 6 cm. standard circle. One of his chicks learned to choose a circle from a triangle, but not when the triangle was inverted.

Johnson (64) trained chicks to distinguish a 30 per cent difference in width of striae, and a difference in angle of 4 minutes. It was very difficult to train them to react to a difference in direction of striae.

Katz and Revesz (66) obtained rectangle-triangle, circle-triangle and circle-rectangle discriminations using the method of placing a glass plate over the forbidden form during training. They trained hens to select only grains having the long axis horizontally with reference to the line of sight, placing grains alternately horizontally and vertically. The hens learned to pick these, and transferred to situations in which only one horizontal grain was presented with three or four vertical grains.

Coburn (16) got a size discrimination of 4.5 against a 5 cm. standard circle in his crows. The discrimination was relative (i.e., they chose the larger of a pair). There was some evidence indicating form discrimination.

Riekel (109) did not succeed in getting a discrimination of

length of line from his hens, but did get one for the size of a square and the size of a sector of a circle. Kroh (**76**) trained hens on triangles, square, circle, pentagon and hexagon. The equilateral triangle used in training carried over to a right and a scalene triangle, and one hen even recognized it with the apex inverted.

Tirala (**131**) has carried on some most ingenious experiments on the role of form in sex recognition. In these he used the canary and a small parrot. Neither of the males were deceived by a two-dimensional representation. The canary went through the characteristic behavior before a stuffed bird, but the parrot was fooled by none of these. Dyeing the feathers of the female also was insufficient to inhibit the sex response, but gluing little bits of paper under each individual feather succeeded.

Revesz (**106**) studied abstraction of form or color, but does not consider "abstraction" the proper word to use. He emphasizes the fact that choice was immediate, not reflective, and would probably agree with Kroh and Scholl (**77**) in their use of the term "reaction to partial content."

Hunter (**61**) mentions that the pigeon runs the maze by vision rather than kinesthesis. Hertz (**48**) remarks that her crow and jackdaw investigated any slight change in their living quarters, but did no experimentation on this question. Bingham (**8**) summarizes the results of experimentation as indicating that the order of discriminability for birds is: size or flicker; brightness or general illumination; and then form.

### *5. Gestalt*

Wissenburg and Tibout (**150**) trained crows to choose the darker of two fields in a Yerkes box. They ask the question, "Is this a reaction to a relation?" They take the attitude that this "is not a choice, but an orientation toward the whole, which in our experiments is constituted by the two lighted fields." They prefer not to consider it a relational reaction.

Lewis (**82**) trained chicks to choose the brighter, middle or darker of three lights. When the chick had learned this, he was transferred to a situation in which all the lights had been increased in intensity, so that the weakest now was as bright as the middle light had been before. The chicks continued choosing on a relational basis.

Revesz (**104**) trained hens to choose the smaller of two figures and found them subject to the vertical-horizontal and Jastrow illu-

sions. Warden and Barr (138) found an apparent influence of the Müller-Lyer illusion in their ring doves, but they did not have a very good discrimination to start with.

Götz (35) trained hens to select the larger of two grains, then put the larger farther away, so that the retinal image of the smaller must have been about twenty times that of the (objectively) larger grain. The latter was still chosen.

Riekel (109) presents some data which, he concludes, do not harmonize with the Gestalt hypothesis in regard to the organization of the retinal field. His hens were trained to peck from the darker of two fields. Then they were presented with a situation in which the fields were presented successively instead of simultaneously: medium gray followed by dark gray meant food; followed by light gray, no food. There was considerable transfer to the new situation. He concludes that the true explanation of choice reactions under these situations is the *Uebergangserlebnis* (sense of transition); e.g., "darkening" means food, "brightening" means no food. There is, however, some evidence for an absolute discrimination as well: if too great a change in the grays was made, the feeding reaction did not occur.

Warden and Rowley (140) attempted to set up an absolute choice for a given gray (No. 26 in the Hering series) as against either a lighter or a darker (No. 1 or No. 50). One dove required 1000 trials to set up 80 per cent correct responses.

Kroh (76) trained hens to take the larger of two grains and found that they would do so independently of color, shape or position of the grains. Present three grains and the two larger would be taken. He concludes that the *Uebergangserlebnis* and the *Zueinander* principles will both have difficulty with these facts. It seems possible that the hens may have reacted to one pair at a time.

### D. Primates

#### *1. Color*

Kinnaman (69) was apparently the first to undertake an investigation of the color vision of the primates. He used food glasses covered with colored papers, and found that choices between colors were easier than between grays. Garner (32) used the preference method, which does not really tell us anything about color vision as such. Shepherd (121) dyed rice different colors and soaked the red grains in quinine. When pink and green pairs were presented

the pink was avoided. He concludes that brightness was not the determining factor.

Watson (144), using a spectral apparatus, set up a fairly good red discrimination, but it was often noted that the monkey seemed not to see in red light. Watson, therefore, prefers to reserve judgment as to red-blindness. Thorndike (130) found that his monkeys could pick a buff diamond from a black one, but had no brightness control.

Furness (31) found that his monkey could match one colored object by another of the same color, but could not carry this abstraction over to other objects of the same color. Kohts (see 161) has studied a chimpanzee extensively by this sample method, and finds him capable of very complex color discriminations. Achromatic choices were much more difficult. Nearness of hue caused more confusion than nearness in brightness.

Revesz (107) reports his experiments on abstraction in monkeys and concludes that no choice was involved.

Haan (40) taught macacus monkeys to choose red, blue, green and yellow from each other and from black, white and thirty grays. He controlled odor, texture, and other possible cues which may arise in the Yerkes multiple-choice box. Kuroda (78) used a similar apparatus and a monkey of about the same phylogenetic standing, and did not succeed in setting up a red-blue-yellow discrimination. Other factors may have entered besides the ability of the animal to make the choice.

### *2. Form and Pattern*

Garner (32) trained his monkey to fit a circle, a square and a triangle into a form board for peanuts. He reports little trial-and-error fumbling in locating the proper opening. Kinnaman (69) got a rough discrimination of different kinds of food containers from his monkeys, as well as discrimination of size. Witmer (151) gave a circus chimpanzee a trial on a form board. The animal quickly learned to put the blocks in the holes, but did not choose the proper holes. He gave only one trial, so there is no evidence as to his ability to learn.

Thorndike (130) made extensive experiments on his monkeys with designs on cards. His brightest monkey learned the following discriminations: YES from N; small circle from small within large circle; two horizontal bars from five; Y from triangle; T from k; white from black diamond; one large dot from three small ones;

horizontal from oblique bar; and length of bar (ratio 2:1). None of these can be taken too freely, since the apparatus used was not free from defects.

Kohts (see 161) got a good form discrimination from her chimpanzee. Johnson (64) found that the monkey can discriminate a difference of 57 minutes in two angles, and of 3 per cent in width of striae. Nellman and Trendelenburg (86) found that M. Rhesus could estimate distance to within two or three centimeters as measured by reaching for food placed outside the cage. The monkey could follow with his eyes a flower pot under which food had been placed when it was rotated with another.

### *3. Gestalt*

Haan (39) has studied the behavior of macacus in more complex perceptual situations. In his first experiment he trained the animal to choose a double circle as opposed to a single circle, to a larger circle, a vertical ellipse and a horizontal ellipse. Correct choices were made when the card was on end and when the distance between the circles was halved. But if smaller circles were used, or squares of approximately equal area, the habit broke down. The power of "abstraction" was limited to changes in spatial relations.

In another experiment he had the monkey choose a 5 cm. line from one 2.5 cm. and one 10 cm. At first choices were made by looking at the locks on the doors, but when this cue was eliminated the animal learned this discrimination. The habit was good if bars were either horizontal or vertical. The differences were gradually reduced until the lines used were 4, 5 and 6 cm. At this point the animal began to react to some cue on the card instead of to the line itself. To test the monkey's ability to choose by texture, etc., Haan took three new cards, having lines the same length, and arbitrarily chose one as correct. The choice was not learned. It is possible that if the original correct card had been kept and two new 5 cm. cards used with it, that the choice would have been made. At any rate, the visual acuity of the macacus is unquestionable.

Gestalt experimentation on the primates has largely tended in the direction of insight into problematic situations rather than in the field of visual phenomena. As a result, a relatively small amount of data is available on the relation of primate to human perceptual behavior. The importance of this type of experimentation rather than the comparatively meaningless experiments on color vision and related problems will be obvious.

The theoretical implications of configurational experiments are not yet clear. As we pointed out above, some of the German experimenters present data which do not harmonize with a theory of retinal organization such as Gestalt has sponsored. From the conditioned reflex experiments of Pavlov and his co-workers, it seems clear that the animal is unable to carry "in mind" a standard in terms of which to determine his reactions. Their results indicate that differential responses develop much more quickly if both positive and negative stimuli are given frequently. This seems to imply solely that standard and variable must be closely contiguous in time and space for the development of an effective differential inhibition. This is what the configuration experiments amount to. When the standard and variable are changed proportionally, no change in reaction can be observed. Readers of current philosophy will be reminded of the argument for the relativity of physical measurements—that if everything, including ourselves, doubled in size, no one would be the wiser. It is, therefore, scarcely surprising that the animal reacts in terms of a present standard rather than a remembered one (in most cases) and if the standard is changed, he is unable to detect that fact. It will be noted that in several of the experiments noted above (e.g., Riekel, **109**; Gayton, **33**; and Warden and Rowley, **140**), the importance of absolute discrimination appeared. That absolute reaction (to an "ideational" standard) is a higher form than the primitive relative or configurational response will be apparent. Absolute discrimination is evidently not without the range of the perceptual equipment of animals, but the differences upon which they have been tested have been too fine to permit the functioning of such. The average human being cannot decide which is the longer, a 5 mm. or a 6 mm. line, unless they are immediately in relation. In the same way, it seems probable that infra-human reactions might be determined by absolute standards, provided only that the differences were great enough to be within their powers. Future experimentation may well be directed along these lines.

## BIBLIOGRAPHY

1. ALLEN, JESSIE, Associative processes of the guinea pig. *J. Comp. Neurol. & Psychol.*, 1904, **14**, 293.
2. BABAK, E., Ueber den Farbensinn des Frosches. *Zsch. f. Sinnesphysiol.*, 1913, **47**, 331–335.
3. BAUER, V., Ueber den Farbensinn der Fische. *Arch. f. ges. Physiol.*, 1911, **137**, 622–627.

4. BAUER, V., Ueber sukzessiven Helligkeitskontrast bei Fischen. *Zentbl. f. Physiol.*, 1909, **23**, 593.
5. BERNARD, H. M., Light sensations of eyeless animals. *Nat. Sci.*, 1897, **10**, 173–180.
6. BILLS, M. A., and MAUKIN, O., Preliminary study of effect of methyl alcohol fumes on brightness discrimination in the white rat. *J. Comp. Psychol.*, 1921, **1**, 495–504.
7. BINGHAM, H. C., Size and form perception in Gallus domesticus. *J. Anim. Behav.*, 1913, **3**, 260–273.
8. BINGHAM, H. C., Visual perception of the chick. *Behav. Monog.*, 1922, **4**, 4 (pp. 104).
9. BLAESSER, AU., Die partielle relative Farbenblindheit der Hühner. *Zool. Jahrb.*,[1] 1926, **43**, 96–120.
10. BREED, F. S., Development of habits in the chick. *Behav. Monog.*, 1911, **1**, 1.
11. BREED, F. S., Reaction of chicks to optical stimuli. *J. Anim. Behav.*, 1912, **2**, 280–295.
12. BUIJTENDIJK, F. J. J., and REMMERS, J., Nouvelles recherches sur la formatión d'habitudes chez les poissons. *Arch. neérl. de physiol.*, 1923, **8**, 165–183.
13. BURKAMP, W., Versuche über das Farbenwiedererkennen der Fische. *Zsch. f. Psychol.* (Abt. II), 1923, **55**, 133–170.
14. CARR, H., and WATSON, J. B., Orientation in the white rat. *J. Comp. Neurol. & Psychol.*, 1908, **18**, 27–44.
15. CASTEEL, D. B., Discriminative ability of painted turtle. *J. Anim. Behav.*, 1911, **1**, 1–28.
16. COBURN, C. A., Behavior of the crow. *J. Anim. Behav.*, 1914, **4**, 185–201.
17. COLE, L. W., Concerning the intelligence of raccoons. *J. Comp. Neur. & Psychol.*, 1907, **17**, 211–261.
18. COLE, L. W., and LONG, F. M., Visual discrimination in raccoons. *J. Comp. Neurol. & Psychol.*, 1909, **19**, 83–107.
19. COLVIN, S. S., and BURFORD, C. C., Color perception of three dogs, a cat and a squirrel. *Psychol. Monog.*, 1909, **11**, 44 (pp. 88).
20. DAVIS, H. B., The raccoon. *Amer. J. Psychol.*, 1907, **18**, 447–489.
21. DEVOSS, J. C., and GANSON, R., Color blindness of cats. *J. Anim. Behav.*, 1915, **5**, 115–139.
22. DUNN, M., The seven senses of fishes. *Contemp. Rev.*, 1899, **76**, 199–219.
23. ERHARD, H., Messende Untersuchung über das Farbensinn der Vögel. *Zool. Jahrb.*, 1924, **41**, 489–552.
24. FIELDS, P. E., Form discrimination in white rat. *J. Comp. Psychol.*, 1928, **8**, 143–158.
25. FIELDS, P. E., White rat's use of visual stimuli in discrimination of geometrical figures. *J. Comp. Psychol.*, 1929, **9**, 1, 107–122.
26. FREYTAG, G., Lichtsinnuntersuchungen bei Tieren. I. Fische. *Arch. f. vergl. Ophthal.*, 1914, **4**, 68–83
27. FRISCH, K. v., Ueber farbige Anpassung bei Fischen. *Zool. Jahrb.*, 1912, **32**, 171–230.

[1] All references to the *Zoologische Jahrbücher* are to the *Abteilung für allgemeine Zoologie und Physiologie der Tiere.*

28. Frisch, K. v., Sind die Fische farbenblind? *Zool. Jahrb.*, 1912, **33**, 107–126.
29. Frisch, K. v., Ueber die Farbenanpassung des Crenilabrus. *Zool. Jahrb.*, 1912, **33**, 150.
30. Frisch, K. v., Weitere Untersuchungen über den Farbensinn der Fische. *Zool. Jahrb.*, 1913, **34**, 43.
30a. Froloff, J. P., Bedingte Reflexe bei Fischen. I. *Arch. f. ges. Physiol.*, 1925, **208**, 261–271.
31. Furness, W. H., Observations on mentality of chimpanzees and orang-utans. *Proc. Amer. Phil. Soc.*, 1916, **55**, 281–290.
32. Garner, R. L., Psychological studies of chimpanzee. *N. Amer. Rev.*, 1905, **181**, 272–280.
33. Gayton, A. H., Discrimination of relative and absolute stimuli by albino rats. *J. Comp. Psychol.*, 1927, **7**, 93–106.
34. Goldsmith, M., Contribution à l'étude de la mémoire chez les poissons. *Bull. inst. gén. psychol.*, 1912, **12**, 161–177.
35. Götz, W., Experimentelle Untersuchungen zum Problem der Sehgrössenkonstanz beim Haushuhn. *Zsch. f. Psychol.*, 1926, **99**, 247–260.
35a. Graber, V., Ueber die Helligkeits- und Farbenempfindlichkeit einiger Meertiere. *Sitzungsber. d. Akad. d. Wissensch. in Wien. Math.-Klasse.* 1885, **91**, 1, 129–150.
36. Gregg, Jamison, Wilkie and Radinsky, Are dogs, cats and raccoons color blind? *J. Comp. Psychol.*, 1929, **9**, 6, 379–95.
37. Gregg, F. M., and McPheeters, C. A., Behavior of raccoons to temporal series of stimuli. *J. Anim. Behav.*, 1913, **3**, 241–259.
38. Greene, W. F., and Laurens, H., Effect of extirpation of embryonic eye and ear on equilibration in Amblystoma. *Amer. J. Physiol.*, 1923, **44**, 120–143.
39. Haan, J. A. B. de, Ueber Wahrnehmungskomplexe und Wahrnehmungselemente bei einem niederen Affen. *Zool. Jahrb.*, 1925, **42**, 272–306.
40. Haan, J. A. B. de, Experiments on vision in monkeys. *J. Comp. Psychol.*, 1925, **5**, 417–455.
41. Haan, J. A. B. de, Versuche über das Sehen der Affen. *Zsch. f. vergl. Physiol.*, 1927, **5**, 699–729.
42. Hadley, C. V. D., Transfer experiments with guinea pigs. *Brit. J. Psychol., Gen. Sec.*, 1927, **18**, 189–224.
43. Haempel, O., and Kolmer, W., Ueber Helligkeits- und Farbenanpassung bei Fischen. *Biol. Zentbl.*, 1914, **34**, 450–458.
44. Hahn, E., Ueber den Farbensinn der Tagvögel und die Zapfenölkügeln. *Zsch. f. wiss. Zool.*, 1916, **116**, 1–42.
45. Hahn, H. L., Some habits and sensory adaptations of cave-inhabiting bats. *Biol. Bull.*, 1908, **15**, 165–193.
46. Hamburger, V., Versuche über Komplementärfarben bei Ellritzen. *Zsch. f. vergl. Physiol.*, 1926, **4**, 286–304.
47. Hara, Y., Der galvanische Hautreflex bei Katzen und Hunden. *Arch. f. ges. Physiol.*, 1922, **195**, 288–290.
48. Hertz, M., Beobachtungen an gefangenen Rabenvögeln. *Psychol. Forsch.*, 1926, **8**, 336–397.

49. Hess, C. v., Ueber den angeblichen Nachweis von Farbensinn bei Fischen. *Arch. f. ges. Physiol.*, 1910, **134**, 1.
50. Hess, C. v., Neue Untersuchungen über den Lichtsinn bei Wirbellosen Tieren. *Arch. f. ges. Physiol.*, 1910, **136**, 282.
51. Hess, C. v., Experimentelle Untersuchungen zur vergleichenden Physiologie des Gesichtsinnes. *Arch. f. ges. Physiol.*, 1911, **142**, 405–446.
52. Hess, C. v., Untersuchungen zur Frage nach dem Vorkommen von Farbensinn bei Fischen. *Zool. Jahrb.*, 1912, **31**, 629.
53. Hess, C. v., Untersuchungen zur Physiologie des Gesichtsinnes der Fische. *Zsch. f. Biol.*, 1913, **63**, 245.
54. Hess, C. v., Neue Untersuchungen zur vergleichenden Physiologie des Gesichtsinnes. *Zool. Jahrb.*, 1913, **33**, 387.
55. Hesse, R., Die Lichtempfindung des Amphioxus. *Anat. Anz.*, 1898, **14**, 556–557.
56. Higginson, G. D., Visual perception in the white rat. *J. Exper. Psychol.*, 1926, **9**, 337–347.
57. Hineline, G. M. W., Color vision in the mudminnow. *J. Exper. Zool.*, 1927, **47**, 85–94.
58. Hinsche, G., Ueber Bewegungs- und Haltungsreaktion bei Kröten. *Biol. Zentbl.*, 1923, **43**, 16–27.
59. Hopkins, A. E., Vision and retinal structure in mice. *Proc. Nat. Acad. Sci.*, 1927, **13**, 488–491.
60. Hubbard, R. M., Stimulus for the visual discrimination habit. *J. Comp. Psychol.*, 1927, **7**, 75–81.
61. Hunter, W. S., Some labyrinth habits of the domestic pigeon. *J. Anim. Behav.*, 1911, **1**, 278–305.
62. Jorden, Hovey, Integumentary sensitivity in a marine fish. *Amer. J. Physiol.*, 1917, **44**, 259–274.
63. Johnson, H. M., Visual pattern discrimination in vertebrates. I. *J. Anim. Behav.*, 1914, **4**, 319–361.
64. Johnson, H. M., Some recent experiments on vision in animals. *Trans. Illum. Engr. Soc.*, 1915, **10**, 502–514.
65. Johnson, H. M., Visual pattern discrimination in vertebrates. II. *J. Anim. Behav.*, 1916, **6**, 169–271.
66. Katz, D., and Revesz, G., Experimentel-psychologische Untersuchungen mit Hühnern. *Zsch. f. Psychol.*, 1908, **50**, 93–116.
67. Katz, D., and Revesz, G., Ein Beitrag zur Kenntnis des Lichtsinnes der Nachtvögel. *Zsch. f. Sinnesphysiol.*, 1913, **48**, 165–170.
68. Katz, D., and Revesz, G., Experimentelle Studien zur vergleichende Psychologie. Versuche mit Hühnern. *Zsch. f. angew. Psychol.*, 1921, **18**, 307–320.
69. Kinnaman, A. J., Mental life of two Macacus Rhesus monkeys in captivity. *Amer. J. Psychol.*, 1902, **13**, 98–148, 173–218.
70. Kittredge, E., Brightness value of red for the light-adapted eye of the calf. *J. Comp. Psychol.*, 1923, **3**, 141–145.
71. Köhler, W., Die Farbe der Sehdinge beim Schimpansen und beim Haushuhn. *Zsch. f. Psychol.*, 1917, **77**, 248–255.

72. KÖHLER, W., Zur Psychologie des Schimpansen. *Psych. Forsch.*, 1922, **1**, 2–46.
73. KOHLRAUSCH, A., and SCHILF, E., Der galvanische Hautreflex beim Frosch auf Sinnesreizung. *Arch. f. ges. Physiol.*, 1922, **194**, 326–329.
74. KRAUSE, W., Die Lichtempfindung des Amphioxus. *Anat. Anz.*, 1898, **14**, 470–471.
75. KRAUSE, W., Die Farbenempfindung des Amphioxus. *Zool. Anz.*, 1897, **20**, 513–515.
76. KROH, O., Weitere Beiträge zur Psychologie der Haushuhn. *Zsch. f. Psychol.*, 1927, **103**, 203–227.
77. KROH, O., and SCHOLL, R., Vergleichende Untersuchungen zur Psychologie der optischen Wahrnehmungsvorgänge. *Zsch. f. Psychol.*, 1926, **100**, 260–273.
78. KURODA, A., Mental life of macacus monkey. *Jap. J. Psychol.*, 1926, **1**, 146–206 (Psychol. Abst., 1927, **1**, 1099).
79. LASHLEY, K. S., Size and form discrimination in the albino rat. *J. Anim. Behav.*, 1912, **2**, 214.
80. LASHLEY, K. S., Color vision of birds. I. The spectrum of the domestic fowl. *J. Anim. Behav.*, 1916, **6**, 1–26.
81. LAURENS, H., Reaction of normal and eyeless amphibian larvae to light. *J. Exper. Zool.*, 1914, **16**, 195–210.
82. LEWIS, M. H., Elemental vs. configurational response in the chick. *J. Exper. Psychol.*, 1930, **13**, 1, 61–75.
83. MAST, S. O., Changes in shade, color and pattern in fishes. *Bull. Bur. Fish.*, 1916, **34**, 173.
84. MUNN, N. L., Pattern and brightness discrimination in raccoons. *Ped. Sem.*, 1930, **37**, 1, 3–34.
85. NAGEL, W. A., Ein Beitrag zur Kenntnis des Lichtsinnes augenloser Tiere. *Biol. Zentbl.*, 1894, **14**, 810–813.
85a. NAGEL, W. A., Der Farbensinn des Hundes. *Zentbl. f. Physiol.*, 1908, **22**, 205–207.
86. NELLMAN, H., and TRENDELENBURG, W., Beitrag zur Intelligenzprüfung niederer Affen. *Zsch. f. vergl. Physiol.*, 1926, **4**, 142–201.
87. PARKER, G. H., Skin and eyes as receptive organs in reactions of frogs to light. *Amer. J. Physiol.*, 1903, **10**, 28–36.
88. PARKER, G. H., On stimulation of integumentary nerves of fishes by light. *Amer. J. Physiol.*, 1905, **14**, 413.
89. PARKER, G. H., Sensory reactions of Amphioxus. *Proc. Amer. Acad. Arts & Sci.*, 1908, **53**, 415–455.
90. PARKER, G. H., Influence of eyes, ears and other allied sense organs on the movements of the dogfish. *Bull. Bur. Fish.*, 1909, **29**, 45–57.
91. PAYNE, F., Reactions of blind fish to light. *Biol. Bull.*, 1907, **13**, 317–323.
92. PEARCE, D. B., Note on the interference of visual habits in the white rat. *J. Anim. Behav.*, 1917, **7**, 169–177.
93. PEARSE, A. J., Reactions of amphibia to light. *Proc. Amer. Acad. Arts & Sci.*, 1910, **45**, 161.
94. POLIMANTI, O., Sur le sens chromatique des poissons. *Arch. ital. de biol.*, 1916, **64**, 293–300.

95. PORTER, J. P., Preliminary study of the psychology of the English sparrow. *Amer. J. Psychol.*, 1904, **15**, 313–346.

96. PORTER, J. P., Further study of the English sparrow and other birds. *Amer. J. Psychol.*, 1906, **17**, 248–271.

97. RAZRAN, H. S., and WARDEN, C. J., Sensory capacity of dogs as measured by the conditioned response method (Russian schools). *Psychol. Bull.*, 1929, **26**, 202–222.

98. REESE, A. M., Light reactions of crimson-spotted newt. *J. Anim. Behav.*, 1917, **7**, 29–48.

99. REESE, A. M., Some reactions of alligator mississippiensis. *J. Comp. Psychol.*, 1923, **3**, 51–59.

100. REESE, A. M., Reactions of snakes to light and touch. *J. Comp. Psychol.*, 1927, **7**, 237–245.

101. REEVES, C. D., Moving and still lights as stimuli in discrimination experiments with white rat. *J. Anim. Behav.*, 1917, **7**, 160–168.

102. REEVES, C. D., Discrimination of light of different wave lengths by fish. *Behav. Monog.*, 1919, **4**, 3 (pp. 106).

103. REICHNER, H., Ueber farbige Umstimmung und Momentadaption der Hühner. *Zsch. f. Psychol.*, 1924, **96**, 68–75.

103a. REIGHARD, J., An experimental field study of warning coloration in coral reef fishes. *Carnegie Inst. of Washington*, 1908, **103**, 257ff.

104. REVESZ, G., Tierpsychologische Untersuchungen. *Zsch. f. Psychol.*, 1921, **88**, 130–137.

105. REVESZ, G., Experiments on animal space perception. *Brit. J. Psychol., Gen. Sec.*, 1924, **14**, 387–414.

106. REVESZ, G., Recherches psychologiques sur les singes. *Arch. neérl. de physiol.*, 1924, **9**, 380–381.

107. REVESZ, G., Experimental study of abstraction in monkeys. *J. Comp. Psychol.*, 1925, **5**, 293–341.

108. RICHARDSON, F., Sensory control in the rat. *Psychol. Monog.*, 1910, **12**, 48 (pp. 124).

109. RIEKEL, A., Psychologische Untersuchungen an Hühnern. *Zsch. f. Psychol.*, 1922, **89**, 81–115.

110. RILEY, C. F. C., Response of young toads to light and contact. *J. Anim. Behav.*, 1913, **3**, 179.

111. ROCHON-DUVIGNEAUD, A., La vision des oiseaux. *Presse Med.*, 1923, **67**, 1397–1405.

112. ROCHON-DUVIGNEAUD, A., Que savons-nous de la vision des animaux? *J. de Psychol.*, 1924, **21**, 730–737.

113. ROUSE, J. E., Mental life of domestic pigeon. *Harvard Psychol. Stud.*, 1906, **2**, 581–613.

114. SACKETT, L. W., Canada porcupine. *Behav. Monog.*, 1913, **2**, 2 (pp. 84).

115. SAMOJLEFF, A., and PHEOPHILATKOWA, A., Ueber die Farbenwahrnehmung beim Hunde. *Zentbl. f. Physiol.*, 1908, **22**, 133–139.

116. SCHAEFFER, A. A., Habit formation in frogs. *J. Anim. Behav.*, 1911, **1**, 309.

117. SCHALLER, A., Sinnesphysiologische und psychologische Untersuchungen

an Wasserkäfern und Fischen. *Zsch. f. vergl. Physiol.*, 1926, **4**, 370–464.

118. SCHIEMENZ, F., Ueber den Farbensinn der Fische. *Zsch. f. vergl. Physiol.*, 1924, **1**, 517–540.
119. SCHILF, E., and SCHUBERTH, A., Ueber das sogennanten psychogalvanischen Reflexphänomen beim Frosch und seine Beziehung zum vegetativen Nervensystem. *Arch. f. ges. Physiol.*, 1922, **195**, 75–95.
120. SCHLIEPER, C., Farbensinn der Tiere und optomotorische Reaktionen. *Zsch. f. vergl. Physiol.*, 1927, **6**, 452–472.
121. SHEPHERD, W. T., Some mental processes of the Rhesus monkey. *Psychol. Monog.*, 1910, **12**, 5.
122. SMITH, E. M., Color vision in dogs. *Brit. J. Psychol.*, 1912, **5**, 119–202.
123. STONE, C. P., Notes on light discrimination in the dog. *J. Comp. Psychol.*, 1921, **1**, 413–431.
124. STRATTON, G. M., The color red and the anger of cattle. *Psychol. Rev.*, 1923, **30**, 321–325.
125. STRATTON, G. M., Cattle and excitement from blood. *Psychol. Rev.*, 1923, **30**, 380–387.
126. SUMNER, F. B., Adjustment of flatfish to various backgrounds. *J. Exper. Zool.*, 1911, **10**, 409–507.
127. SUTHERLAND, A. H., Complex reactions of the dog. *Psychol. Monog.*, 1917, **23**, 100, 241–265.
128. SZYMANSKI, J. S., Lernversuche bei Hunden und Katzen. *Arch. f. ges. Physiol.*, 1913, **152**, 307.
129. SZYMANSKI, J. S., Ueber die Fähigkeit der Hunde zur Bildung von optischen Assoziationen. *Arch. f. ges. Physiol.*, 1918, **171**, 317–324.
130. THORNDIKE, E. L., *Animal Intelligence*. New York, 1911.
131. TIRALA, L. G., Die Form als Reiz. *Zool. Jahrb.*, 1923, **39**, 395–442.
132. TORELLE, E., Response of frog to light. *Amer. J. Physiol.*, 1903, **9**, 466–488.
133. TUGMAN, E. F., Light discrimination in English sparrow. *J. Anim. Behav.*, 1914, **4**, 79–109.
134. VANHEUSEN, A. P., Skin of catfish as receptive organ for light. *Amer. J. Physiol.*, 1917, **44**, 212–214.
135. VALENTINE, W. L., "Visual perception in the white rat." *J. Comp. Psychol.*, 1928, **8**, 369–376.
136. VERRIER, M. L., Le sens visuel chez les vertèbres. *J. de Psychol.*, 1929, **26**, 74–100.
137. VINCENT, S. B., The white rat and the maze problem. *J. Anim. Behav.*, 1915, **5**, 1–24, 367–374.
138. WARDEN, C. J., and BAAR, J., Müller-Lyer illusion in the ring dove. *J. Comp. Psychol.*, 1929, **9**, 4, 275–292.
139. WARDEN, C. J., and AYLESWORTH, M., Relative value of reward and punishment in formation of visual discrimination habit in the white rat. *J. Comp. Psychol.*, 1927, **7**, 117–128.
140. WARDEN, C. J., and ROWLEY, J. B., Absolute vs. relative brightness in the ring dove. *J. Comp. Psychol*, 1929, **9**, 5, 317–337.

141. WARDEN, C. J., and WARNER, L. H., Sensory capacities and intelligence of dogs. *Quar. Rev. Biol.*, 1928, **3**, 1–28.

142. WASHBURN, M. F., and ABBOTT, E., Experiments on the brightness value of red for the light adapted eye of the rabbit. *J. Anim. Behav.*, 1912, **2**, 145–180.

143. WASHBURN, M. F., and BENTLEY, M., Establishment of an association involving color discrimination in the creek chub. *J. Comp. Neurol. & Psychol.*, 1906, **16**, 113–125.

144. WATSON, J. B., Some experiments bearing upon color vision in monkeys. *J. Comp. Neurol. & Psychol.*, 1909, **19**, 1.

145. WATSON, J. B., Studies on the spectral sensitivity of birds. *Carnegie Inst. of Washington*, 1915, **211**, 85–104.

146. WATSON, J. B., and WATSON, M. I., A study of the responses of rodents to monochromatic light. *J. Anim. Behav.*, 1913, **3**, 1–14.

147. WAUGH, K. T., Role of vision in mental life of mouse. *J. Comp. Neurol. & Psychol.*, 1910, **20**, 549–599.

148. WHITE, G. M., Association and color discrimination of minnows and sticklebacks. *J. Exper. Zool.*, 1919, **27**, 443–498.

149. WILLIAMS, J. A., Form perception and learning in dogs. *Comp. Psychol. Monog.*, 1926, **4**, 18, 1–70.

150. WISSENBURG, J. C., and TIBOUT, P. H. C., Choix base sur l'aperception complexe chez les cobayes. *Arch. neérl. de physiol.*, 1921, **6**, 149–162.

151. WITMER, L., Monkey with a mind. *Psychol. Clin.*, 1909, **3**, 179–205.

152. WOLFF, H., Das Farbenunterscheidungsvermögen der Ellritze. *Zsch. f. vergl. Physiol.*, 1925, **3**, 279–330.

153. WOOD, C. A., Color sense of satin bower bird. *Amer. J. Ophthal.*, 1925, **8**, 120–122.

154. YERKES, R. M., Mutual relations of stimuli in the frog. *Harvard Psychol. Stud.*, 1906, **2**, 545–574.

155. YERKES, R. M., Space perceptions of tortoises. *J. Comp. Neurol. & Psychol.*, 1904, **14**, 17.

156. YERKES, R. M., *The Dancing Mouse.* New York, 1907.

157. YERKES, R. M., Modifiability of behavior in its relation to age and sex of the dancing mouse. *J. Comp. Neurol. & Psychol.*, 1909, **19**, 227–232.

158. YERKES, R. M., Color vision in the ring dove. *Proc. Nat. Acad. Sci.*, 1915, **1**, 117–120.

159. YERKES, R. M., and EISENBERG, A. M., Preliminary to a study of color vision in the ring dove. *J. Anim. Behav.*, 1915, **5**, 25–43.

160. YERKES, R. M., and MORGULIS, S., Method of Pavlov in animal psychology. *Psychol. Bull.*, 1909, **6**, 257–273.

161. YERKES, R. M., and PETRUNKEVITCH, A., Studies of chimpanzee vision by Ladygin-Kohts. *J. Comp. Psychol.*, 1925, **5**, 99–108.

161a. ZOLONITSKY, N., Les poissons, distinguent-ils les couleurs? *Arch. zool. exper. gen.*, 1901, 3e ser., **9**, 1–5.

162. ZOTH, O., Beitrag zur Beobachtungen und Versuchen an japanischen Tanzmäusen. *Arch. f. ges. Physiol.*, 1901, **86**, 147.

163. HONIGMANN, H., Untersuchungen über Lichtempfindlichkeit und Adaptierung des Vogelauges. *Arch. f. ges. Physiol.*, 1921, **189**, 1–72.

INDEX

| *Problem* | *Fish, etc.* | *Birds* | *Mammals* | *Primates* |
|---|---|---|---|---|
| Color Vision | 2, 3, 13, 22, 26, 27, 28, 29, 30, 34, 35a, 43, 46, 49, 51, 52, 53, 54, 57, 83, 94, 102, 103a, 117, 118, 120, 126, 143, 152, 161a. | 9, 10, 11, 23, 44, 67, 68, 71, 77, 80, 95, 96, 103, 104, 113, 145, 153, 158, 159, 163. | 1, 17, 18, 19, 20, 21, 36, 37, 59, 70, 114, 115, 122, 124, 125, 128, 142, 146, 147. | 31, 32, 40, 41, 69, 71, 72, 78, 106, 107, 121, 130, 144. |
| Reaction to Light | 5, 30a, 38, 50, 55, 58, 62, 73, 74, 75, 81, 85, 87, 88, 89, 90, 91, 93, 98, 99, 100, 110, 119, 132, 134, 154. | 48. | 45, 47. | |
| Brightness | 12. | 133. | 6, 60, 84, 92, 97, 101, 108, 123, 127, 129, 137, 139, 141, 156, 157, 162. | |
| Size, Form and Pattern | 15, 116, 155. | 7, 8, 10, 16, 61, 63, 64, 65, 95, 105, 131. | 14, 24, 25, 56, 63, 64, 65, 79, 84, 97, 135, 141, 149. | 69, 86, 130, 151. |
| Gestalt | 4. | 35, 66, 76, 82, 138, 140, 150. | 33, 42. | 39. |

## SPECIAL REVIEWS

EDWIN G. BORING. *A History of Experimental Psychology.* N. Y. & London: Century Co., 1929. Pp. xvi+699.

It is not without significance that the title of this book begins with the indefinite article; in more than one passage the author makes it clear that in his opinion his work is *a*, and not *the*, history of experimental psychology; and his opinion is justified. Any book, even a history, must of course reflect its author's scholarship. The present book is, in addition, warm with its author's personality. Facts, for him, are real and their discoveries exciting; the biographies are accounts of human beings; not of names—part titles of books and monographs—but of men who struggled and contended, who had their disappointments and triumphs. The style of this history, which is neither literary nor journalistic but rather that of an excellent conversational lecturer, glows with an enthusiasm that, however much it may wax and wane, is never extinguished. There is no question that the book is addressed, not to the author's colleagues, but to his graduate students; there is an occasional dogmatism which the student accepts and the colleague resents; there are reiterations which the student doubtless needs but which the colleague finds a bit tiresome. There is no question either but that there are charts on the wall—one a genealogical tree of physiologists and psychologists and their pupils, together with their dates, another a geographical map of the European universities. The latter is reproduced on the inside covers of the volume.

The words "experimental psychology" mean for the author "what they meant to Wundt and what they meant to nearly all psychologists for fifty or sixty years—that is to say, the psychology of the generalized, human, normal, adult mind as revealed in the psychological laboratory" (viii).[1] Its history, then, "deals with the psychology of a half-century from 1860 to 1910, with its preceding development and its consequences—a spindle-shaped history, as it were" (viii). Other histories of psychology have, it is said, "presented the experimental movement as . . . the termination of the long development of philosophical thought about mind" (vii).

[1] The numbers in parentheses refer to pages in the History.

They all "present psychology more as an outgrowth and branch of philosophy than as an experimental science" (176n.). The author however sees two lines of descent: "debouching from Descartes, Leibnitz, and Locke on the philosophical side, and developing within the new experimental physiology of the early nineteenth century on the physiological side. It was of the union of these two movements that experimental psychology was born" (viii). In the sequel we shall see how successful the author has been in developing this view.

Experimental psychology is also regarded as a science; and from the first to the last paragraphs of the book its author is enmeshed in the relation between science and philosophy. His discussion is not sufficient to make his conception of the relation clear. In the first place, he apparently seeks to avoid not only the time-honored distinction that the logical method of science is primarily induction whereas that of philosophy is deduction, but also the more recent view that science is in essence descriptive whereas philosophy is interpretative. At any rate, neither is mentioned even by implication. Instead, a search is first made for the origin of certain principles that are characteristic of science: such, for example, as the principle that nature can be explained internally without reference to external agency, the principle of analysis, the dilemma of change and permanence, and the problem of scientific law. The beginnings of all these, and consequently of science, are found in early Greek philosophy. "Thales appears as almost the first known scientist because, in the face of the fixed habits of mythological explanation in his time . . . he nevertheless conceived the notion of looking for another kind of explanation of nature within nature itself" (7). The author finds however two differences between philosophy and science. First, the former is rationalistic (6), it employs the *a priori* method (22); the latter is observational (14), it employs the experimental method (8), although "science is quite as rational as philosophy" (6, 14). From this point of view Thales, who "was an astronomer," was not a scientist; he "did not contribute to the observational method" (9f.). The method of Socrates and Plato was also rationalistic; "they were more interested in reasoning about nature and in hypothesizing its fundamental principles than in observing it; and indeed, when any distinction can be drawn between philosophy and science, it is presumably this difference between the focusing of interest upon reason and its focalization upon observation. Actually, the difference is never more than a difference of emphasis, for the philosopher cannot

ignore experience, nor can the scientist proceed except rationally by way of observed relationship and hypothesis. The difference is real, however, and in some men and in some historical periods it has become very great. Socrates and Plato were in temperament philosophers, not scientists" (6). We may overlook the difficulty of explaining how a difference that is "never more than a difference of emphasis" may nevertheless be a "real difference;" but we cannot avoid noting that the author has in the course of these few lines shifted his ground from the distinction between science and philosophy to a difference in temperament among men who work in the two fields.

Secondly, despite the above quotation, the author seems also to find a difference in kind. It is, if I understand him, that science is objective and philosophy is subjective. In a paragraph which follows directly upon a discussion of Aristotle, who is found not to have realized the primacy of observation, it is said: "On the whole, Greek philosophy remained essentially objective, and a sharp distinction between philosophy and science did not appear until a subjective philosophy emerged with John Locke" (7). Does the author forget that Bacon in the *Novum Organum* had made, before Locke was born, a distinction which to this day is the one accepted by most men of science and is not altogether repudiated by many philosophers? In its place a distinction is suggested which as it stands is highly questionable. Is philosophy limited to the subjective? Is psychology a philosophy, or is it a science? The author himself in a later passage throws some light on the latter question. "With Locke," we are told, "philosophy became predominately psychological, thus making psychology philosophical and not scientific" (153). It is apparently meant that with Locke, and within philosophy, psychology became an important and overtly recognized part of philosophy (154). It was this psychology then that was subjective. The first question remains unanswered. Science, we may suppose, is objective; philosophy since Locke has centered upon epistemological problems, and in this sense may be regarded as subjective. Philosophy, however, is not now and never has been limited to the subjective; and Experimental Psychology, it is said, is scientific. Did the latter become objective? The author, as I have said, is not clear, and we must wait and see how these distinctions work out in the subsequent narrative.

We turn then to the development of the two streams, and first to that of "physiological psychology." Boring's account of the

movement in physiology during the nineteenth century is the freshest and most masterful of the book. The work of Bell, Magendie, Johannes Müller, Claude Bernard, and Helmholtz on the physiology of the nervous system; of Gall and Spurzheim on phrenology; of Flourens, Broca, Fritsch, and Hitzig on brain physiology; of Müller and the doctrine of specific nerve energies; the experimental physiology of sensation and perception; the history of hypnotism; and finally the history of the "personal equation," which of course developed in astronomy rather than in physiology—all are excellently told. There are some errors of fact, which we shall for the present ignore, and one important error in historical perspective. The author reads his own conception of psychology into that of the physiologists. True, he himself says, "the men who made these discoveries did not call themselves psychologists" (153f.). Nevertheless, he calls them psychological physiologists, he seems to take it for granted that terms like sensation and perception meant for them what they later meant for psychologists; he has entitled Chapter 6 The Physiological Psychology of Sensation; and finally he finds about 1860 "two psychologies"—the one the rationalistic psychology of Locke and his followers, and the other the psychology of the physiologists. "What more natural than that these two 'psychologies' should fuse and become one?" (154). The same slip in historical perspective occurs later on in the book. "Helmholtz," it is said, "did not of course 'found' experimental psychology, for he thought of psychology as essentially physiological, and physiology as essentially physical. Had the question been raised he would have opposed a 'mentalism' just as he did oppose a vitalism. Psychology was for him an exact science, dependent upon the use of mathematics, as Herbart had tried to show, and upon experiment, which Herbart denied" (295). Boring has again read his own conception of psychology into that of Helmholtz. The latter did not regard psychology as essentially physiological. His psychology was a mixture of the brand that had descended from Locke and, as the author states, of John Mill's logic. Helmholtz distinguished outer and inner sensations. The latter are psychological and can be studied only by inner perception. The former, on the other hand, are the direct result, the "effects," of nervous excitation; they can be directly observed, measured, and studied by physiology; and as qualities of perception they are signs or tokens (not copies) which furnish the material, as it were, for unconscious inference to work upon. Helmholtz was trying to prove that per-

ceptual knowledge is ultimately based upon physiology. He had also however to take the *mental acts* of memory and inference into account. But he expressly stated that it was the problem of pure psychology, and not his problem, to determine the nature of these acts. The author's further treatment of Helmholtz is admirable. I am personally grateful for the suggestion that Helmholtz' view of the unconscious inference "is merely the principle of conscious decay under habituation" (302), although of course the unconscious inference was, as the conscious decay is not, a psychic act.

I return to the "two psychologies," and in particular to the author's further treatment of philosophical psychology prior to the union of the two streams. He begins with Descartes, Leibnitz, and Locke, although these are preceded by four pages on Aristotle. In the succeeding chapters there follow accounts of the psychologies of Berkeley, Hume, and Hartley; of the Mills, Bain and Spencer; and finally of Herbart and Lotze. In the Notes the reader is told that these chapters deal "with philosopher-psychologists—not completely, but only as they form the pertinent background of modern psychology in general and experimental psychology in particular" (203). It is fair therefore only to ask whether the items selected are indeed pertinent, whether they are fairly stated, and whether there are serious omissions. As regards the first of these questions the author has a good rule. "It is not enough to note mere anticipations of later thought. We have here the actual beginnings of later theories in the sense that the development of thought was continuous and the lines of influence are more or less clear" (168). On the whole he has lived up to this rule; the first chapter of the book and the section on Aristotle suffer, it is true, from "mere anticipations," and occasionally "the lines of influence" are a bit forced, due apparently to a fondness for analogy in which likenesses are stressed too frequently at the expense of greater differences.

In the four pages on Aristotle the enthusiasm of the author is at its lowest ebb; he really wanted to begin with Descartes; "the history of thought is however continuous and no history of any kind of psychology can quite avoid the mention of Aristotle's name" (155). So the debt to the great man is paid, the anticipations are merely listed, and no lines of influence are found. What became of the history of thought during the two thousand years between Aristotle and Descartes? Had the psychology of the middle ages no influence upon modern psychology? In one respect at least it had a very great influence. For it was from the Scholastics that Brentano

got his term "intentional inexistence" for characterizing the "act," and the Scholastics got the "act" from Aristotle. Thrice Boring refers to Brentano's psychology as Aristotelian, and he correctly points out that Stumpf, Meinong, Witasek, Husserl, the later Külpe, and Messer—all derive from Brentano; and yet in the discussion of Aristotle's psychology there is no word of the "act." A serious omission! But what is more serious, the author instead finds the "line of influence" for the act-psychology in Leibnitz's concept of mental activity (167). Leibnitz, however, is not even mentioned in the Brentano of 1874, and—so far as this connection is concerned—for the very good reason that the activity of the "act" is its least important characteristic; Husserl has even denied that it has any activity. It is of course correct to say, as is indicated, that the doctrine of activity is important in much of modern psychology, but it is incorrect to say, as the author says, that it had its beginning in Leibnitz; it is as old as the doctrine of the soul. The "principle of unity" also, which, by another false analogy, is linked with Gestalt psychology, had its beginnings with neither Leibnitz nor Descartes, but goes back to the Middle Ages, if not to the Greeks. Similarly, as good an anticipation of physiological psychology as the author finds in Descartes will be found in Galen. I appreciate fully the author's problem. But if he is tracing lines of influence, either he should have left Aristotle out of his history and commenced if necessary with Descartes and Leibnitz, together with the statement that these philosophers represent at the beginnings of modern philosophy the culmination of ancient and medieval doctrines of the soul. Or else, if Aristotle must be brought in, the psychological philosophers of the Middle Ages should also appear in the history.

Taken as a whole, the accounts given of philosophical psychologists are well done. The selections from their philosophy are made deftly and are presented clearly and interestingly if not always accurately. Minor errors I shall again for the present ignore. It is not perhaps a serious error to make Descartes deny souls to animals outright, but, as Washburn long ago pointed out, it is a common error. In the author's account of the associationists' theories of objects there runs a mistake which is more vital because, as it seems to me, it reveals a want of insight into certain fundamentals of British associationism. "It is plain," he writes, "that this discussion has landed us in an *associative theory of meaning*, very similar to the modern context theory (Titchener) *if not identical with it*. We met it at first in Berkeley, it has kept turning up since.

Now we find it in even clearer form in James Mill" (215, latter italics mine), and it crops up again in John Mill (222). In the section on Berkeley it is also said: "To the author it seems that Berkeley's theory of objects is a direct anticipation of Titchener's theory of meaning, and that both imply association without naming it" (185). The likenesses here are greater than in other of the author's analogies, but the differences are so great as to preclude the least possibility of an identity. Let us consider these differences. For the associationists meaning was intrinsic to sensation. Consequently Berkeley could say, "that which is seen is one thing and that which is felt is another . . . the true consequence is that the objects of sight and touch are two different things" (*A New Theory of Vision,* Par. 49). The coach seen, the coach heard, and the coach felt (in the passage quoted from Berkeley by Boring, p. 185) are three different coaches, and Berkeley was puzzled to explain how, in common speech, they are referred to as the same coach. For Titchener, on the other hand, meaning is not intrinsic to mental process; consequently he could say, "one and the same meaning may attach to several experiences" (*Beginner's Pyschology,* 29). The three experiences mean one and the same coach; the content-processes are different, but the meaning put upon every one is the same. Furthermore, if it is said that both Berkeley and Titchener imply association without mentioning it, it should also be said that the two men did not mean the same thing by "association." For Berkeley it was an explanatory principle; for Titchener, it was only a generalized statement of what, under certain conditions, happens. In so far as Berkeley had a context theory the doctrine of association was necessarily implied, but for Titchener it was not, because it was no part of his problem to account for the context. In a sense, of course, the accrual of the context to the given process is itself a kind of association, but whether it falls under the law of association as Titchener stated the law is a question that, so far as I can see, must be left to experimentation. Is then the author justified in suggesting that Berkeley's theory of objects is a *direct* anticipation of Titchener's context theory? From what has been said the answer is unequivocally no. But it may have been an indirect anticipation. Titchener did not make up the theory out of whole cloth; he knew his Berkeley as he also knew his Helmholtz, Hering, Wundt, and James, and the experimental investigations of perception. In visual space-perception Berkeley's "awareness of the position of the eyes" gave way in the early experimental period

to sensations of eye-movement; and eye-movements were employed in different ways by Hering the nativist, Wundt the geneticist, and Helmholtz the empiricist. In Titchener, however, when he admitted eye-kinaesthesis as part-content in a particular perception, it appeared as a special case of context in the general theory; and his formulation would, as far as it goes, have been equally acceptable to Berkeley, Helmholtz, Hering, or Wundt. It is therefore a long and indirect way from Berkeley to Titchener.

The end of the account of the philosopher-psychologists is reached with a bit of relief. More than a third of the history is behind us. We may now expect to get the two psychologies united, then pick up the thread of experimentation where it was dropped at the end of the story of the physiologists, and see what psychology has in its own behalf accomplished by way of discovery. It may however be said once and for all that no such expectation will be realized. The title of the book is misleading. It should have read: A History of the Antecedents and Course of Systematic Psychology, and Trends of Psychological Investigation since the Beginning of the Experimental Period—or something more elegant but no less indicative. We turn then to a section devoted to the founding of experimental psychology, which deals with the lives and works of Fechner, Helmholtz, Wundt, Brentano, Stumpf, and G. E. Müller. The "lives" are interestingly portrayed; some of them, particularly those of Stumpf and Müller, contain biographical material that is new. The accounts of the systematic contributions of these psychologists are, as a rule, uninspired digests. The warmth the author feels for the man is wanting for his ideas. The insight, the realization of the true inwardness of the movements in psychological thought at this period, the illumination that will make the various systematic conceptions alive and reasonable are wanting. Brentano, for example, deliberately turned his back upon the new experimental movement; he, together with the physiologists of his time, regarded sensation (content) as physiological (physical), and it was his purpose to solidify and thus to perpetuate what we now call the philosophical tradition; nevertheless the author places him among the founders of experimental psychology. One may even question the presence of Stumpf and Müller in this list, but it is not so important. There is however one question which I hoped the author might raise and answer: Why did Stumpf call his book *Tonpsychologie* and not *Tonphenomenologie*? For, what experimental psychology has taken from the book is not what Stumpf

offered as psychology. The answer will show how much nearer Stumpf was to Brentano than to the new psychology. Despite the author's appreciation of Müller and of his experimental work, I do not think his systematic contributions are evaluated at their true worth. The *Gedächtnistätigkeit* is a mine yet unworked, and taken with the *Komplextheorie* is a restatement of associationism that purges it of many of those attributes which have made the term one to be feared by undiscriminating psychologists. Müller's doctrine of association should have been brought into relation with the British tradition on the one side and that of Wundt and his followers on the other.

We return to Fechner and Wundt, who, the author had previously stated, were the real founders of the movement. With these two, we were told, "there was a formal, conscious recognition of the essential identity of the two psychologies, and since the philosophic member had been called 'psychology' and the scientific member 'physiology,' the birth of 'physiological psychology' was a natural consequence and of the utmost importance in the history of psychology" (154).

When however we come to the introduction of the section on Fechner we find instead that he was a product of psychological physiology and "a philosophical belief in a scientific or physiological psychology: Herbart, Lotze, Hartley, Bain" (265). The two terms scientific and physiological psychology are not here, as elsewhere, intended as correlative, for Herbart, as the author states, got along without either physiology or experiment. It was the notion of psychology as science that the author thinks Fechner got from Herbart. The term *Wissenschaft* in Herbart's title *Psychologie als Wissenschaft, etc.* is translated "science," *i.e.,* as if Herbart meant the word *Wissenschaft* in the sense of *Naturwissenschaft*. Boring may, in this instance, be correct, for Herbart himself is not clear. Fechner however acknowledges an indebtedness to Herbart; the influence of the others mentioned is doubtful. Lotze's book did not, as the author later states, appear until after the *Zend Avesta*, in which Fechner's conception first appeared; Bain did not publish the *Senses and Intellect* until 1855, four years after Fechner had begun his experiments; and I know of no evidence that Fechner had ever heard of Hartley. All this is in the short introduction to Fechner. In the discussion of Fechner himself, however, Boring finally makes the more accurate statement: "his psychophysics, the sole reason for Fechner's inclusion in this book, was, as we have implied, a by-

product of his philosophy" (270). Would it not have been less confusing, and perhaps better history, to have said this in the first place; and also to have said that Fechner himself thought he was founding a new science which he called psychophysics, that we have Wundt's word for it that Fechner was not in the least interested in psychology, and that psychology nevertheless has appropriated his experimental procedures and statistical devices as a means of mental measurement, and thus he is honored as one of the founders of experimental psychology? All of this the author eventually says or implies, but his introduction, his guiding threads, are entirely misleading.

The situation with Wundt is similar. Instead of finding him formally recognizing the identity of the two psychologies, we read that the *Beiträge* revealed "Wundt, while still immersed in physiology, beginning to think and work in experimental psychology" (315); that from Herbart he got the idea that psychology must be a science, though for Wundt the term "scientific" meant experimental (315); that in 1873–74 the *Grundzüge* appeared, which on the one hand was a "symbol of his metamorphosis from physiologist to psychologist and, on the other hand, was the beginning of the new '*independent*' science" (317, italics mine); that Wundt (327) "argued that psychology is not the science of inner experience" (thus, I may add, rejecting the most fundamental tenet of philosophical psychology since Locke); that he distinguished psychology from physics by point of view (although we are not told that he was thereby able to claim sensation as content-process for psychology and at the same time keep sensation from another point of view for physiology); and finally, but not until the end of the book, in a masterful summary of the history we are told that "Wundt took the nervous system from the physiologist, vision and audition from Helmholtz, certain fragments of experimental physiological psychology from Weber, psychophysics and Weber's Law from Fechner, the reaction experiment from the astronomers, associationism from the British psychological philosophers; he bound them together with a systematic logic which he contributed, and he made a new psychology" (656). All of which is true, although the final quotation is only pictorially correct; nervous system is not subject-matter of psychology for Wundt, and he got more from sense physiology than Helmholtz and Weber had to give. If, in view of all this, the author will still insist that Wundt represents no more that a "fusing of physiological and philosophical psychology," that he merely

"recognized the essential identity of the two psychologies," it is because he reads his own meaning of psychology into the physiology of the time, he reads his meaning of physiological psychology into Wundt, and he fails of insight into what Wundt, with all his cumbersome, much discredited systematic scaffolding, was really trying to do. He even fails to grasp the inwardness of Wundt's system. In one passage referring to the *Grundzüge* of 1874 it is said, "It was a systematic handbook in both senses of the word; it was built about a system of psychology, and it *attempted systematically to cover the range of psychological fact* . . . Wundt did not write another, more mature system of psychology: he modified, improved, and expanded the original. It was called a Physiological Psychology" (317, italics mine). In another passage the author writes "Wundt never held that the experimental method is adequate to the whole of psychology: the higher processes, he thought, must be got at by the study of human nature, his *Völkerpsychologie*" (322); and in still another place, after mentioning the ten volumes of the *Völkerpsychologie,* he says, "It is possible that, if Wundt had been younger, we should have had a seventh edition of the *Physiologische Psychologie* with the results of this labor carefully integrated with the rest of the system" (326). It would, however, have been quite impossible for Wundt to have included the results of the *Völkerpsychologie* in the *Physiologische Psychologie.* By the term physiological psychology he meant only as much of psychology as could be attacked by the methods of physiology. Thought, as the author says, could be attacked only by the historical method. How then could he bring the topic of thought into the *Grundzüge?* It was in the *Vorlesungen* that Wundt dealt with psychology as a whole.

The next division of this History, almost a half of the book, is devoted to "Modern Experimental Psychology." It might better have borne the title, "The Lives and Works of Modern Psychologists." In the beginning Ebbinghaus, Külpe, and Titchener are treated according to the method of earlier sections. Thereafter the narrative becomes progressively less a history and more a chronicle. The author justifies his method on the ground that "The 'new' psychology was intensely self-conscious and personal" (413). The result is disheartening; psychology is lost in a medley of personalities. True, in the course of the narrative the author takes account of "form-quality," of the problem of mental inheritance and statistical method (in which Galton's influence upon psychology receives proper recognition), of the influence of the doctrine of Evolution

upon psychology, and of the origin and development of Functional Psychology in America (which is a distinct contribution). There are also brief historical accounts of Mental Tests, and of Animal Psychology, of Behaviorism, and Gestalt Psychology. In all of these psychology lifts its head above the personalities, only to be lost again in a multitude of proper names, dates, titles of books and monographs, and fields of experimentation. It is all interesting and informing and useful, but psychology seems to be getting nowhere. In the end the author himself confesses to a disappointment at the outcome of the 70 years' progress of experimental psychology. He is convinced that "the application of the experimental method to the problem of mind is the great outstanding event in the history of the study of mind" (659). He is also certain that experimental psychology is a science. What then is the trouble? In the first place, he thinks psychology needs a great man like Helmholtz or Darwin to lead it out of the wilderness. In the second place, as he concludes in the final paragraph of the history, "Psychology ought to fare better when it can completely surrender its philosophical heritage, in fact as well as in voiced principle, and proceed, unimpeded by a divided soul, about its business" (661). Are these not strange words from an author who in the beginning of his book declared that "Fundamentally knowledge is one, and it may be that philosophy and science are again approaching each other" (153); that if philosophy is rationalistic, science is also rational; that "when any distinction can be drawn between philosophy and science, it is presumably this difference between the focusing of interest upon reason and its focalization upon observation" (6)? One is tempted to say that the author has accepted too easily the all-knowledge-is-one dictum of the philosopher and consequently is a bit surprised to find "something incompatible between experimentation and philosophizing; they belong together in principle but do not blend in the personality" (520f.). One wonders if he could possibly have confused the term "rationalistic," a technical term in epistemology, and "rational," a common term for logical thinking. But however one may try to explain it, the fact remains that we are told both that science and philosophy are essentially the same, and that psychology, a science, should give up philosophy. The only way out is to give up the one or the other of the two statements. Personally, I think there is a fundamental difference between science and philosophy and that a scientific psychology should be on its guard against philosophy. I cannot admit, however, as the author seems to say,

that all systems of psychology thus far written are at bottom philosophical. He correctly states that Wundt, Ebbinghaus, Külpe in the *Outlines,* and Titchener tried to avoid philosophy, and he further implies that they did not succeed. Wundt is charged with "writing both philosophical psychology and the psychology of philosophy" (656). Ebbinghaus' reference to Mach and Külpe is ignored by the author, but his psychology is said to be "of the Wundtian tincture, somewhat diluted by tolerance and by a dislike of philosophical argument" (386). Külpe and Titchener are said to have leaned upon Avenarius and Mach for "epistemological support in their definition of psychology" (615), and "this underlying 'epistemology' of Titchener's was, of course, not philosophically sophisticated" (412). We are also told that "to Külpe and Titchener he [Mach] seemed to establish the validity of introspection as a scientific method" (391). The author does not cite a single reference in support of these statements, and I know of none. They are all gratuitous. The charges against Külpe and Titchener are most specific, and I shall limit myself to them. These men were searching for a definition of psychology, a verbal formula that would logically, not epistemologically, set off psychology as a science from physics. Külpe first found it in Avenarius and Mach, who, working from different epistemological considerations, happened to be in practical agreement on that point. Four years after Külpe published the *Outlines,* Ebbinghaus also adopted the same definition; and sixteen years after Külpe, Titchener did the same thing, although in the meantime he had published two text-books in both of which he had defined psychology in another way. But in accepting the formula neither Külpe, Ebbinghaus, nor Titchener committed himself to the epistemological doctrine of either Avenarius or Mach. Why should they? In a scientific treatise a logical distinction may be, and in these cases was, made without necessarily implying or requiring justification of any epistemological or metaphysical theory. The test is to ask whether the distinctions and assumptions are fully adequate to the systematic organization of the facts of psychology. The author has fallen into the method of some philosophers, who must first classify a writer under some epistemological category and then proceed to criticize him in the light of their classification. By so doing he has been unfair to the very men who, with expert knowledge in both science and philosophy, made the greatest effort to free psychology from its "ancient heritage" and believed that they accomplished it. It would have been fairer to have taken them at

their word and to have attempted their exposition in accordance with their declarations.

Regarding the history as a whole, I think it will do more to stimulate an interest in the history of psychology than any book thus far written, and it will also serve as an exceedingly useful compendium of historical fact in an easily-read and interesting form. I do not think, however, that the author has succeeded in justifying his view that experimental psychology is the conjunction of two streams, the one philosophical, the other physiological psychology. Upon his showing, experimental psychology is for much the greater part still the handmaiden of philosophy. He has never made clear his own thinking as regards the difference between science and philosophy, and accordingly he has not justified his own psychological faith. The student should, I think, be warned against a too ready acceptance of the author's generalizations; and he should be on guard against the author's analogies, occasional slips in historical perspective, and numerous small errors. I conclude with a list of corrigenda. Some have been supplied by the author and some by my colleagues. The phrasing will identify those that express my own alternative opinions.

| | | |
|---|---|---|
| Page 8, | line 9. | Instead of "does" read "do." |
| Page 20, | line 17. | To "sought to confute him" it would certainly be more accurate to add "in part." |
| Page 31(1),[2] | line 31. | I should add that Whewell takes care of Leibnitz and Bacon in *The Philosophy of Discovery*, where, of course, in his three-part history of science they belong. |
| Page 46(1), | line 24. | Instead of "250,000" read "2,500,000." |
| Page 90, | line 35. | Blix first published (in Swedish) in 1883. |
| Page 90, | line 36. | Blix did not discover pain-spots. |
| Page 97, | line 28f. | I should add, W. Nagel, *Handbuch der Physiologie des Menschen.* |
| Page 102, | line 20. | Instead of 1684 read 1798, the date usually given for Dalton. The paper was read in 1794. |
| Page 103, | line 4. | To "the horopter must be a circle" I should add "when the eyes are converged." |
| Page 104, | line 39. | Instead of "phenomona" read "phenomena." |
| Page 136, | line 30. | Instead of Struve read Argelander. |
| Page 148(2), | line 49. | Instead of Pierce read Peirce. |
| Page 160, | line 11. | Instead of "animals lack souls," read "lack rational souls." |
| Page 161, | line 9. | Instead of "no souls," read "not rational souls." |

[2] The numbers 1 and 2 in parentheses refer to the 1st and 2d columns of the page indicated.

| | | |
|---|---|---|
| Page 168, | line 8. | Not strictly correct. (See p. 177, col. 2, bottom of page.) |
| Page 169, | line 39. | Instead of 1670 read 1671, the more probable date. |
| Page 170, | line 15. | Instead of Shaftsbury read Shaftesbury. |
| Page 178(2), | line 15. | Instead of Shaftsbury read Shaftesbury. |
| Page 180, | line 29. | Berkeley arrived at Newport but did not sail for it. |
| Page 204(2), | line 31. | Instead of Bermudan read Bermudian. |
| Page 209, | line 32. | Instead of 1818 read 1817. |
| Page 217, | line 24. | Instead of "twelve" read "eleven." |
| Page 233, | line 27. | After "animals" interpolate "rational." |
| Page 248, | line 1. | Instead of "is" read "it." |
| Page 266, | line 38. | Instead of "not quite" read "had just." |
| Page 270, | line 17. | Instead of "philosopsy" read "philosophy." |
| Page 292, | line 35. | It would, I think, be more accurate to interpolate "following Faraday and Maxwell" after "Hertz who," and delete "the first." |
| Page 308(2), | line 47f. | Instead of "der" and "in" read "den" and "im." |
| Page 320, | line 39. | Instead of "der" read "die." |
| Page 324, | line 29. | The sentence is ambiguous. To convey the author's meaning the word "all" should receive the stress; or else, instead of "all" read "some." |
| Page 346, | line 4. | Instead of "Trendelenberg" read "Trendelenburg." |
| Page 361, | line 31. | Instead of "Trendelenberg" read "Trendelenburg." |
| Page 365, | line 28. | In Jost's own formulation of his laws he, after "equal strength" adds "but of different age." |
| Page 390, | line 6. | Instead of 1885, read 1886. |
| Page 407, | line 35. | Instead of "attribute of" read "element similar to." |
| Page 415, | line 18. | Instead of 1878 read 1872–74. If the "revised edition" refers to the *Grundzüge der Lehre vom Lichtsinn*, it is, I should say, a new book, not a revision of the *Lehre*. |
| Page 425(1), | line 25. | Instead of 18th read 8th. |
| Page 426(2), | line 33. | Instead of 1885 read 1886. |
| Page 433, | line 40. | Instead of "as" read "by way of." I should regard it as unfortunate if the reader should from this passage infer that there was any essential difference between Külpe and Titchener on this point. |
| Page 443, | line 1. | Instead of "was only one kind" read "were two kinds." |
| Page 443, | line 2. | After "acts" read "and contents." (See table p. 448.) |
| Page 443, | line 27. | I should say that, in this context, Witasek belongs with Külpe and Messer. I am, however, not sure of the meaning of this passage. |
| Page 446, | line 35f. | I cannot agree with the author concerning both statements within the parenthesis. |
| Page 447, | line 2f. | The passage will be clearer if instead of "these three" one reads "these last three," *i.e.* the three characteristics mentioned under (3). |

| | | |
|---|---|---|
| Page 448(2), | line 42. | Instead of "cosmogeny" read "cosmogony." |
| Page 466, | line 18. | Instead of "(1864– )" read "(1864–1929)." |
| Page 479, | line 26f. | Delete "It is no wonder . . . at Oxford." Sherrington did not come to Oxford until 1905. |
| Page 500, | line 25ff. | Appropos of this passage compare E. B. Titchener, *Systematic Psychology: Prolegomena*, 182 n. |
| Page 505, | line 29. | Instead of Trendelenberg read Trendelenburg. |
| Page 529, | line 2. | Instead of "immediately" read "two years later." |
| Page 529, | line 6. | Instead of Pierce read Peirce. |
| Page 531, | line 29. | Instead of Freiberg read Freiburg. |
| Page 537(1), | line 41. | Instead of Pierce read Peirce. |
| Page 551, | line 22. | Instead of "emphatically" it is obvious that one should read either "empathetically" or "empathically." |
| Page 562, | line 34. | Instead of "invented" read "employed." |
| Page 568(1), | line 21. | After "anticipated" interpolate "to a great extent." |
| Page 568(1), | line 33. | It should, I think, be remarked that Köhler says "practically all the observations were made in the first six months of 1914." Apparently he did little research during the war (Cf. *The Mentality of Apes*, 1926, 7 n.). His first paper from the station appeared in 1915. Yerkes published a monograph on *The Mental Life of Monkeys and Apes* in 1916. |
| Page 595(2), | line 39. | Instead of 1924 read 1914, 2d ed., 1921. |
| Page 615, | line 1. | Instead of 1882 read 1881. |
| Page 615, | line 15. | Instead of 1885 read 1886. |
| Page 617, | line 28. | Blix is Swedish, not German. |
| Page 618, | line 2. | Instead of 1884 read 1883. |
| Page 618, | line 4. | Instead of 1885 read 1884. |
| Page 618, | line 31. | Instead of Pierce read Peirce. |
| Page 626, | line 10. | Instead of Pierce read Peirce. |
| Page 647, | line 5. | Instead of Bannister read Banister. |
| Page 647, | line 7. | Instead of 1923 read 1920. |
| Page 648, | line 16. | Instead of W. de Abney read W. de W. Abney. |
| Page 648, | line 19. | I should delete "a revision of the older book." |
| Page 673(1), | line 1. | Instead of Abney W. de, read Abney W. de W. |
| Page 673(2), | line 12. | Instead of Bannister, H., read Banister, H. |
| Page 680(1), | line 21. | Instead of Pierce, B. read Peirce, B. |
| Page 680(1), | line 22. | Instead of Pierce, C. S. read Peirce, C. S. |
| Page 682(1), | line 1. | Instead of Trendelenberg, read Trendelenburg. |

*Cornell University.* H. P. WELD.

*The Foundations of Experimental Psychology.* Edited by CARL MURCHISON. Worcester, Clark University Press, 1929. Pp. 907.

This important book, presented under the editorship of Professor Carl Murchison, is written by a series of specialists. In this review

each article will be considered separately, and at the end of the paper a statement will be made concerning the general significance of the volume for psychology.

1. *The Mechanism and Laws of Heredity.* By T. H. Morgan.

In this article one of the foremost experimental geneticists attempts to answer the question of the significance of the science of genetics for psychology. He shows in the first place that there is no satisfactory evidence tending to support the Lamarckian inheritance of psychological traits. The nature of Mendelian heredity is next presented with admirable clarity. But a number of reasons are given to show why man makes very poor genetic material. These difficulties are particularly obvious in regard to the so-called psychological traits because almost every such characteristic, even in the young child, is complicated by individual experience. The contributions and the shortcomings of the statistical work of the biometricians upon the problem of heredity are considered. It is suggested that a study of human identical twins may supply much information on the relative importance of heredity and environment in the development of human nature. A list of 217 titles is appended.

This article is a valuable and important contribution to psychology. As the reviewer has elsewhere pointed out, there is too great a disposition among psychologists to deal with the matters of human mental inheritance by *analogy* with the facts known concerning the color of garden peas. Dr. Morgan shows the fallacy of such an approach. His criticism of the statistical work in this field is cogent and timely.

2. *The Study of Living Organisms.* By W. H. Crozier.

This article is an explicit effort to treat animal behavior as a department of physics. By quantitative experimentation alone, the author holds, is the scientific knowledge of living organisms to be advanced. The tropism doctrine offers an interesting approach to the quantitative study of the facts of behavior. The tropistic reactions of the mealworm, the young rat, a type of shell-less pulmonate, the slug, the tent caterpillar, certain marine planarians, and other types have been studied. Some of the movements of these organisms can be shown experimentally to depend upon conditions susceptible of quantitative study. When quantitative constants are determined, it is possible to describe in mathematical terms certain behavior acts as functions of stimulus situations. A study of

genetically stabilized young rats of varying races is given as an example. Each race shows characteristic behavior on an inclined surface. Prediction of the behavior of crosses of these races has proved possible. In any effort to account for behavior in quantitative terms the problem of "spontaneous movements" arises. A number of observations are given in regard to the genesis of such movements in the body of the organism. A bibliography of 142 titles is appended.

The reviewer believes that the work here summarized is of the utmost importance for one who would have an understanding of animal, and indeed of human, behavior. It should be expressly stated, however, that in spite of the somewhat polemic tone of the writer the methods suggested by him are not the only truly scientific methods which may be applied to the study of conduct. The author prefers to make his presentation distinctly mathematical, but the reviewer believes that there are comparatively few psychologists or students of psychology who are, as the author playfully suggests, "paralyzed by the sight of a mathematical formula." Certain behavior situations are too complex for ready mathematical treatment. For example, as the author himself points out, the problem of orientation when three sources of light are presented is practically insoluble as an exercise in mathematics. Physiologists and psychologists who believe in the ultimate machine-like nature of the organism still may wish to start the analysis of behavior from other points than those chosen by the author.

3. *The Mechanism of Reaction.* By Alexander Forbes.

The nature of the mechanism of response is here approached from the evolutionary standpoint. The author gives the various stages in the phylogeny of the neuromuscular mechanism. The structure and function of the adjustor mechanism is outlined. The classes of effectors are next treated. The process of conduction is shown to be related to that of excitation, and the theories accounting for the development of the propagated disturbance as it is seen in the nerve and muscle are presented. The evidence in regard to the refractory and supernormal phases is summarized. The All-or-None law is considered, and the law itself is restated in clear terms. The evidence in regard to "decremental conduction" of the nerve in a narcotized area is considered. Metabolism and fatigue in the nerve are treated, as is also the nature of muscular contraction. The author brilliantly presents the possibility of treating the functions

of the central nervous system in terms discovered in the experimental study of the isolated neuron fiber. The author well says, "The scope of this idea is large. A reduction of the elements of neural activity underlying consciousness and behavior to the single basis of the nerve-impulse, many of whose physical properties are now known, would be a generalization comparable to the reduction of all the various chemical elements to their constituent protons and electrons." In developing this significant idea certain of the work of Sherrington is reviewed, and the phenomena of conduction time, after-discharge, summation, irreversibility, fatigue, variability of threshold, dependence on blood-supply and susceptibility to anaesthetics, reinforcement, and inhibition, are considered. The paper ends with a consideration of tonus and a restatement of the fact, accepted by most psychologists as a truism, that a vital force need not be assumed to account for the nature of response. It is interesting to note that new as is this section of the book, some of the most recent work of Adrian has already changed certain statements as presented. A bibliography of 116 titles is appended.

The reviewer feels that this article should be commended in the highest terms. It is a clear and scholarly treatment of some of the most important phenomena that are indeed, as the title of the volume has put it, at the foundation of experimental psychology.

4. *Vision: I. Visual Phenomena and their Stimulus Correlations.* By L. T. Troland.

In this chapter Dr. Troland clearly presents the principal facts of the psychophysics of vision together with a number of additional observations. He begins his treatment with a consideration of the nature of the experiential factors in vision, and the relationship between visual sensory and perceptual experience. The structure of the psychological color solid and the various modes of color appearance, such as surface, film, luster, and the like, are considered. The nature of the visual stimulus and the structure of the eye are next presented. On the basis of this consideration the psychophysics of brilliance and of the chromatic attributes of hue and saturation are ably and clearly treated. The binocular relations of color, the nature of visual form, the nature of depth, and the visual perception of color are in turn discussed. The amount of space devoted to an exposition of the last named factors is less than that given to the discussion of the psychophysics of visual sensory experience. A brief notice is given to the relationship between the retina and the cortex. That the retina has a point-to-point representation on the

calcarine surface is stated as a fact. A bibliography of 76 titles is appended.

In this treatment Dr. Troland has summarized many of the important facts of vision which concern the psychologist. The consideration of the relationship between the various characteristics of the stimulus and the nature of visual experience seems to the reviewer a brilliant piece of work.

5. *Vision: II. The Nature of the Photoreceptor Process.* By Selig Hecht.

This chapter deals with the nature of the receptor process, specifically in the eye, but the nature of receptor activity in general is also treated. The function of the photoreceptors may for convenience' sake be divided into those factors which are concerned with intensity and those which are concerned with color. These two factors are treated independently in the discussion. All sense organs are shown to be in certain factors alike. Speaking functionally, the "outside end" of a receptor is so organized that it can be upset by one typical energy of the environment. Its "middle part" is similar in all receptors and concerns the quantitative relation between the receptor process and the stimulating environment. The "inside end" is alike for all receptors, and is concerned with the initiation of the sensory nerve impulse. The concept of threshold is considered in its relation to the Bunsen-Roscoe law. On the basis of the time and intensity relationship in long and short exposures, and certain other factors, the essential duality of the visual sensory process is discussed. The dual relationships noted here are shown to hold in elaborate studies of the reaction time to light of the clam and of other relatively simple organisms. On the basis of these observations the fundamental physio-chemical picture of the photoreceptor process is held to involve two definable aspects: (1) a primary system, and (2) a "dark" system. The relationship of the facts thus established to the visibility of color at various intensities and to the Weber-Fechner law is considered. A treatment follows of the relationship between visual acuity and illumination. The importance of the All-or-None law and of the frequency hypothesis of Adrian is here considered. To the reviewer this seems a most promising solution of the insistent problem raised by the apparent conflict between the all-or-none principle and the observed facts of intensive change. The treatment of color vision is less detailed than that of intensity. The Newtonian facts of color mixture are reviewed and the adequacy of the Young-Helmholtz, the

Hering, and the Ladd-Franklin theories is briefly considered. In regard to the last two theories the author holds that the facts of binocular color mixture (for example, he notes that red in one eye and green in the other give yellow) invalidate these theories. The phenomenon of binocular mixture requires that mixture be central, while older theories assume peripheral mixture. It is probable that more experimental work will have to be done on this problem before it is finally settled. A bibliography of 83 titles is added.

The reviewer finds this paper significant. It is particularly important because of the light which it throws upon what may be termed the Neo-Müllerian doctrine of the specificity of receptor process.

6. *Hearing: I.* By H. Banister.

The author first reviews briefly and clearly the physics of the auditory stimulus and then turns to consider in order the nature of tone, the limits of hearing, pitch, the minimum and maximum intensities that lead to auditory experience, and the various auditory difference limens. The distinction between tone and noise, the pitch of pure and compound tones, intertones, combination tones, beats, and "binaural beats," auditory after-sensations, and consonance and dissonance, are treated in turn. In the discussion of the last two phenomena the theories of Helmholtz and Stumpf are emphasized, but it is pointed out that the changes which music has undergone in recent years have a distinct bearing on the problem. The auditory perception of space is considered, but it is pointed out that in such space many extra-auditory factors enter. The facts established in regard to the physical basis of localization are reviewed. Some space is devoted to a consideration of the argument concerning the so-called attribute of volume in sound. Speech sounds, the nature of deafness, and the characteristics of auditory fatigue are considered. A bibliography of 59 titles is appended.

This chapter is a valuable summary of some of the more important facts of the psychology of audition. It is surprising to the reviewer not to find in the chapter reference to Halverson, Pratt, Seashore, Ogden, and many other workers. These omissions are no doubt due to the limitations set by the pedagogical aim of the chapter.

7. *Hearing: II.* By H. Hartridge and H. Banister.

This chapter is devoted to a consideration of the anatomy of the ear and the various theories of audition. The literature concerning the structure and function of the external, middle, and inner ear is

reviewed. It is alleged that summation and difference tones depend upon the action of the conical drum. The telephone theories, the sound pattern theories, the sound wave theories, the volume hypothesis, and the summation hypothesis are considered. The evidence in favor of a theory of the general type of that expounded by Helmholtz is ably brought forth. Anatomy, pathology, animal experimentation, the evidence of physical models, the sharpness of resonance, the persistence of vibration, and many other phenomena are all considered in such a manner as to show definite support for a resonance theory. The hypotheses of Watt and of Boring are summarized and the major difficulties of these theories as seen by the authors are exposed. The authors attempt to rebut the major objections to the resonance theory as raised by Mayer, Keith, Boring and others. A bibliography of 48 titles is appended.

The treatise here reviewed is able and valuable, but is surprising for what it omits rather than what it includes. To take but a single example, no reference is made to any of the more than 25 papers of C. E. Seashore on audition, and yet complaint is made by the authors that references to tonal islands have "not proved traceable." Recent work has rendered certain statements in the article out of date.

8. *The Chemical Senses.* By G. H. Parker and W. J. Crozier.

This chapter begins with an evaluation of the words commonly used in sensory psychology and physiology. The disadvantages of the subjective terms, the clumsiness of the objective vocabulary, and the grotesqueness of the partly subjective, partly objective terminology are presented. The evolution of the chemoreceptive sense is treated as an introduction to a consideration of the nature of the stimuli, the structure and the function of the receptor organs, and the nature of the experiences, presented in olfaction, in the common chemical sense, and in taste. In each case the character of these senses is considered in a comparative manner. The nature of the chemical stimulus for taste is considered in detail. A bibliography of 113 titles is appended.

This chapter ably summarizes a large literature. It treats certain topics in much greater detail than others of equal interest. In some respects the limitations of space have made this article inferior to the previous publication of the senior author, that model among monographs, *Smell, Taste, and Allied Senses in the Vertebrates.*

9. *The Sense of Feeling.* By J. P. Nafe.

According to the author of this section of the volume the sense of feeling includes all of the experiences that are commonly "felt" in the popular meaning of the word. The exposition involves an effort to correlate stimulus, receptor process, and psychological phenomena. A quantitative theory of feeling is built up upon the basis of the modern work of Adrian and his school upon the nature of the nervous impulse. The author dispenses with the older view of various skin qualities, although he presents the evidence that in the past led to the acceptance of the classical view. Even the senses of temperature and pain are considered in a quantitative rather than a qualitative manner. Localization, perception, and the discrimination of two points, are treated as essentially acquired capacities, and the perceptual complexes, such as wetness and stickiness, are mentioned. Movement, acuity, and adaptation, are each considered in turn. Kinaesthetic experience, visceral sensitivity, and the general affective qualities are briefly treated. A list of 61 references is given.

Every time the reviewer turns to a consideration of the skin senses, he is tempted to give up words and merely write a series of exclamation points. Of all topics in physiological psychology it is possibly the most difficult one in which to disentangle facts from theories: to separate the true from the false. The chapter here presented is radical. It is stimulating. The author may be very nearly all right or he may be very nearly all wrong, but the chapter that he has prepared is certain to bring about that sort of experimental investigation by which alone the problems of the skin senses are to be solved.

10. *Space and the Non-auditory Labyrinth.* By James Q. Holsopple.

In this article the emphasis is placed upon the nature of the problem that faces one who is interested in the functions of the non-auditory labyrinth. Some of these problems are concerned with questions of whether or not there is space perception that cannot be explained without reference to the labyrinth. The nature of perceptions which originate in the labyrinth, the morphology of the receptor and its mode of stimulation, the characteristics and modification of the behavior that follows vestibular stimulation, are questions that cannot at present be completely answered. The literature of the sense is often inaccurate, and where it is valid it relates to infrahuman rather than human labyrinths. The author points out that the main bodily effects of the stimulation of the vestibule are

changes in the tonicity of antagonistic muscles. Nystagmus and past-pointing are important because they exemplify such changes. The nature of the laboratory apparatus useful in vestibular research is considered. A list of 16 titles is appended, among which are articles in which complete bibliographies are presented.

This article is a valuable summary of the experimental work that has been done upon certain aspects of vestibular stimulation. It also presents frank statements of the difficulties of investigation in this field. Some apparently relevant literature is not considered. For example, no special reference is given to the work of de Kleijn or Magnus, although some of this work is on the human individual, and much of it is by inference important for an understanding of the bodily responses to labyrinthine stimulation.

11. *Hunger and Thirst.* By W. B. Cannon.

The author points out that in most of its essential processes the body is self-regulating. Food and water are necessary to the body and they are both continually being lost. It is therefore essential that a constant supply of both be maintained. Various theories which have been proposed to account for the maintenance of the proper supply of food and water are considered. In the first instance hunger and thirst are defined as sensations, while appetite is characterized as the desire for the repetition of the pleasurable experience that results from previous indulgence. Experiment has shown that the old central sensation theories of hunger are not valid, but that hunger has a local origin in the movements of the stomach. The exact cause of these contractions of the stomach, however, is not settled, but a number of attractive chemical laws have been proposed to account for the behavior. The experience of thirst is referred to the inner surface of the mouth and throat. Experiment shows that secretion of the salivary glands is reduced by a deprivation of water. This is shown in both human and animal experiments. Indeed, it seems that the sensation of thirst is not directly due to a lack of fluids in the body, but results from a local condition of dryness in the mouth. Any stimulation which increases the secretion of the salivary glands removes the sensation of thirst, whether or not the water content of the body is changed. When long unsatiated the stimuli for hunger and thirst become the masters of the organism. A list of 19 titles is appended.

To the reviewer this treatment is a paradigm of its kind. It is terse, summary, and above all clear.

12. *Emotion: I. The Neuro-humoral Basis of Emotional Reactions.* By Philip Bard.

After a short but informative historical summary the author turns to a discussion of the structure and function of the autonomic nervous system. The general biological significance of the autonomic system is next considered, and the emergency theory of Cannon is explained. The complexity and interdependence of the processes which follow autonomic stimulation are described. The central nervous system mechanism for emotional reactions is considered. Evidence is presented which leads the author to the conclusion that emotional responses are subcortical. It is asserted that the behavior of a rigid cat is not due to the cortex, but to sympathetic neural connections. The author holds that certain behavioristic psychologists have "grossly disregarded" these phenomena. The James-Lange theory is reviewed and severely criticized upon the basis of certain experimental and observational tests. It is alleged that experiments demonstrate that visceral factors are needed to explain the genesis of emotion, and that the afferent impulses from the skeletal muscles do not play the important part ascribed to them by many modern supporters of the James-Lange theory. The view of Cannon that the peculiar quality of the emotion is added to the simple sensation when the thalamic processes are aroused is ably presented and supported. A bibliography of 59 titles is appended.

This article is informative. It is written by one who knows intimately both the literature and the experimental procedure of the field with which he is dealing. The reviewer must admit, however, that he still has a very large blind spot in regard to why there should be an effort to shift the point of emphasis in the study of emotion from the periphery to the thalamus. Such a procedure seems calculated to obscure and not to clarify the understanding of emotion.

13. *Emotion: II. The Expressions of Emotion.* By Carney Landis.

This chapter begins with a statement of the possible methods of experimentation upon emotion and with a definition of the major terms to be used in the chapter. The criteria of emotion are next considered, after which a brief historical summary of the works on emotional expression is presented. As part of the consideration of vocal expression the theories that have been proposed to account for

laughter and crying are summarized. The bodily reaction that is the emotion or that accompanies the emotion is next dealt with. In this section blood volume, blood pressure, blood changes, heart reactions, respiratory reactions, metabolic rate, and gastro-intestinal activity are considered. The place of the endocrine glands in an understanding of emotion is next presented. The functions of the suprarenal, thyroid, and pituitary bodies, and gonads are considered in some detail. The relationship between the nervous system and the expression of the emotions is considered briefly. This section may well be described as a supplement to the previous section by Dr. Bard. A suggestion is made in regard to the possible rôle of chronaxie in the affective life. A sane and valuable history and evaluation of the so-called psycho-galvanic reflex is given. Brief consideration is given to the relationship between emotion and "feeling." The chapter ends with a suggestion of the probable lines of future development of experimentation in emotion. A list of 49 references is appended. This list of references is not intended to be a complete one. Almost a page of the text is given to a review of Wundt's theory of emotion, but no reference is given to any of Wundt's works.

The reviewer would like to characterize this as one of the most successful chapters of the book. The subject of emotion and affection is complex and highly controversial, and yet Dr. Landis has organized the material and evaluated it in a manner that is satisfactory. There is one very small point, however, with which the reviewer would like to take exception. He does not like reference to sensory impulses that pass over the autonomic nervous system. As Dr. Bard points out on a previous page of the same book, it is probably best to say that "the autonomic nervous system is made up entirely of motor neurons."

14. *Learning: I. Nervous Mechanisms in Learning.* By K. S. Lashley.

The nature of modification by experience is a most important topic for psychology. In the present chapter Dr. Lashley considers the development of behavior patterns during growth and certain other physiological processes concerned in an understanding of learning. He briefly reviews the physiological theories of the growth of the nervous system, particularly in so far as an understanding of this growth bears upon the understanding of the instinct and maturation theories. A comparative consideration of the learning ability

in the animal series shows that little evidence is presented to show that there has been any development in the rate of formation of simple habits in the course of phylogenetic development. The process of learning in the lower centers of the central nervous system is considered. Evidence concerning the localization of the engram is next presented, together with a discussion of the function of the neuron and the synapse in learning. The common elements in transfer are not, in the author's opinion, common neurons in the remaining nervous tissue, but common ratios of excitation in different neural systems. Evidence of an exceptional and clinical sort bearing upon the problem of the localization of function in the cortex is considered, and there follows a discussion which leads to an understanding of the relationship between peripheral and central processes in habit. The so-called laws of learning are reviewed and criticized as are the physiological theories proposed to account for the modification of behavior. The paper ends with a formulation of the problems of learning. A bibliography of 42 titles is appended.

In this short paper Professor Lashley has brought together the results of his own striking experimental work and the results of other investigators in his own and allied fields. The chapter is rich in the facts that it presents and stimulating in the criticism which it levels and which it will arouse.

15. *Learning: II. Experimental Studies of Learning.* By Walter S. Hunter.

In this chapter the experimental work that has been done on acquisition and retention is ably reviewed and systematized by Professor Hunter. After showing the wide range of the field, the author turns to consider the nature of those relationships which are graphically represented on the learning curve. In this section the Vincent curve, the nature of plateaus, and the characteristics of so-called insight are treated in a notable manner. The conclusion is reached that "no causal factor of insight has been demonstrated, and that resort to such a hypothesis is to desert the straight-forward experimental attack upon the determining conditions of behavior from which alone an adequate solution can be derived." The problem of whole versus part learning is considered, as is the related question of the distribution of effort. The complications introduced by the relationship between growth and age factors and learning are treated. The work of Thorndike upon adult learning, the work of Woodworth, Norsworthy, Pyle, and Lyon upon the relationship between speed of learning and retention, is summarized. The influ-

ence of active and passive recall upon retention and the influence of activity upon previously established habits is considered. Experimental work upon forgetting, transfer of training, the laws of learning, and a number of other related topics is reviewed. An excellently selected bibliography of 123 titles is appended.

Factual material, criticism, evaluation, and able organization, has made this treatise, in the opinion of the reviewer, truly excellent.

16. *The Individual in Infancy.* By Arnold Gesell.

Dr. Gesell begins this chapter by pointing out the fact that infancy is not merely a prologue to life, but that it plays an important part in the total career of an individual. The complexities of the present calendar, as well as the fact that children are born after prenatal periods of varying lengths, make age a difficult problem for the exact student of children. It is suggested that the ontogenetic zero be placed at conception and that development be considered successively in the germinal stage, the embryonic stage, the foetal stage, the stage of early infancy, the stage of later infancy, the stage of early childhood, and that of later childhood. The rest of the paper is devoted to an illuminating presentation of the methods and results of investigation in the periods indicated above. The nature of the pattern of behavior is considered; the importance of co-twin control is emphasized; consideration is given to studies of pre-natal behavior, perceptual behavior, motor development, language behavior, learning, emotion, personality, and other factors. In presenting this work ample reference is made to the literature of the subject, and the highly significant results of the author's own institute, the Yale Psycho-Clinic are briefly summarized. A bibliography of 32 references is appended.

This chapter presents a coherent and scientific picture of the period of development. It is stimulating in that in certain respects it is rather a suggestion of what remains to be accomplished than the record of past achievement. In its totality the chapter is a significant account of a phase of psychology all too often ignored by the experimental psychologist. The mental life of the adult must in many ways remain enigmatical to one who is unacquainted with the ontogeny of behavior and experience.

17. *The Individual in School: I. General Ability.* By Rudolf Pintner.

Tests lie on the borderline between pure psychology and "psychological engineering." In the present article an able statement is given in almost outline form of some of the most generally accepted

facts in regard to intelligence tests. Some consideration is given to the various efforts that have been made to define and explain intelligence, and particularly to the work of Thorndike and Spearman in this connection. The distribution and growth of intelligence are considered. The author seems to favor the view that general intelligence and other mental traits are inherited, but he avoids dogmatism on the point. A list of 81 relevant references is given.

This short and able summary should prove of real value to any one who desires a terse statement of the development and present status of the so-called " intelligence testing movement."

18. *The Individual in School: II. Special Abilities and their Measurement.* By Frank N. Freeman.

The author suggests that two general types of individual differences may be distinguished: (1) Those of temperament, and (2) those of ability. The differences in ability may be considered as differences of general ability and differences of special ability. The present chapter is devoted to a consideration of the psychology of special ability. After clearing the way by making certain preliminary definitions particularly in regard to the distinction between aptitude and achievement, the origin and development of special aptitude tests and of educational tests is considered. The process of test standardization is next explained and criticized. The question of scoring, of norms, and of validity, are respectively treated. The uses of educational tests are considered, and their limitation indicated. In conclusion, the point is made that abilities are not simple entities but are usually made up of measurable components. An excellent bibliography of 70 titles is appended.

The chapter presents in a critical and informative manner a summary of the best work that has been done in the testing of special abilities. There can be no question of the scientific validity of this work, but there are some psychologists who may consider it rather in the province of experimental education than in that of scientific psychology.

19. *The Adult in the Community.* By M. A. May.

In this paper the author explains the relationship as he sees it between experimental psychology and the psychology which studies the individual in the community. He further points out that the study of the individual differences which exist in the early years of life are better known than are those of adult years, because babies and school children cannot so easily keep themselves out of the

clutches of inquisitive scientists as can adult human beings. The intimate relationship existing between differential and social psychology is pointed out. The author then proceeds to consider what he terms "Differential Social Psychology." Under this section biological background, the effect of environment, the analogies discoverable in animal societies, and differential adult psychology are considered. Under the last heading much quantitative work on human differences is summarized. The remainder of the chapter is devoted to "Integral Social Psychology." Here the evolution and factors concerned in adult social living are briefly but ably considered. A bibliography of 43 titles is appended.

This chapter is a valuable consideration of the present status of one of the sciences dealing with the individual in the social environment. Incomplete as is the work in this field, the present able and scientifically toned chapter is a definite contribution to psychology, and a contribution that should be read and pondered by many "philosophizing" sociologists.

20. *The Conflict and Survival of Cultures.* By Clark Wissler.

In this chapter evidence from psychology, history, sociology, anthropology, and economics, is brought to bear upon the question of the nature of cultures and their development and change. The reaction of cultures, borrowing, group dominance, and leadership, are considered as leading up to the major topic of the chapter, namely, the conflict of cultures. Under this heading a large number of social customs are considered, and it is pointed out that when cultural change takes place by the mixing of two or more cultures, the result may lead to the degradation of the subject group, and of the individuals of the group. The place of magic is briefly considered, as are also a number of other social phenomena. A bibliography of 15 titles is appended.

In this chapter a brief and informative article is given in regard to certain of the bases of social life. As yet this field has not been studied by methods that are to be characterized as scientific in the narrow sense of the term, but the author expresses it as his belief that such study will develop. The reviewer feels that at certain points in this chapter the distinction between fact and analogy is not made clear to the reader.

21. *The Abnormal Individual.* By Shepherd Ivory Franz.

According to the author there are four large groups of mentally abnormal people: (a) the feeble-minded, (b) the psychotic, (c) the

neurotic, and (d) the eminent or geniuses. These various groups are defined and a particular effort is made to show the relationship between each group and the development of the brain and other bodily structures. Some consideration is given to the fundamental philosophical basis which is knowingly or unknowingly held by those who advocate the "mind-twist" or the "brain-spot" theories of mental disorder. The various conditions which lead to cortical inefficiency are considered, and the more important types of mental abnormality are discussed. In spite of the large amount of evidence that is given in regard to physical change in mental disease, it is significant that the author concludes his paper with the statement, "Unfortunately we cannot associate the mental abnormalities with definite cerebral changes." A classified bibliography of 38 titles is appended.

In this chapter a sane and balanced outline of certain aspects of abnormal psychology is given by a student who really knows the brain and the abnormal patient. The presentation is of great value.

22. *General Statistical Principles.* By T. L. Kelley and Eugene Shen.

Statistical methods, these authors hold, are primarily intended to summarize and analyze data obtained from a series of observations. In the present chapter the statistical series, errors in data, methods of graphing and tabulation, measures of central tendency, measures of dispersion, errors of sampling, and measures of studying relationship and correlation are considered.

This chapter presents in a terse form some of the more essential principles of statistics which are important for the student of experimental, educational, or social psychology.

23. *The Statistical Treatment of Certain Typical Problems.* By T. L. Kelley and Eugene Shen.

This chapter is in reality a continuation of the one noted above. It deals first with partial correlation, multiple correlation, and curve-fitting. A section is then devoted to the psychophysical methods. Here a brief review is given of the method of limits, the method of average error, and the method of constant stimuli. In considering the latter method, some ingenious suggestions are made in regard to the relationship between the classical methods of quantitative psychology and the newer statistical methods that have largely been developed in the service of applied psychology. The methods of

reliability and validity and several related topics are also considered. In conclusion, there is a critical reference to the work of Spearman. A bibliography of 51 titles is appended. This bibliography also serves as a list of references for Chapter 22.

This chapter is, like the preceding, a brief but valuable presentation of some of the mathematical techniques fundamental particularly in those fields of psychology which are not subject to experimental control.

*General Evaluation of the Volume.* The editor has certainly performed a valuable service to psychology in conceiving and editing the present volume. All of the papers, as the separate comments above indicate, are of high quality. The book is important for the professional psychologist and for the serious beginning student who wishes to understand psychology as a natural science.

The reviewer feels, however, that the scope of the volume may possibly be at once too broad and too narrow. Had two volumes instead of one been prepared, the result might have been more satisfactory. Chapters one to sixteen have a certain unity about them, as have chapters sixteen to twenty-three. The first volume might well have been termed the *Foundations of Experimental Psychology,* the second, the *Foundations of the Psychology of Individual Differences.* If such a division had been made it might have been possible to treat of certain important topics which, presumably because of lack of space, have been omitted from the present volume.

The editor promises new editions of the book, so that it may always be abreast of progress. This is certainly much to be desired, and it is the sincere hope of the reviewer that nothing may interfere with the plan. The day of the comprehensive treatises on scientific psychology by one man will never pass, but it is certainly more and more important to have such general handbooks supplemented by detailed, summarized, and authoritative statements by experts. All those who are interested in the science of mental life are indebted to the editor of *The International University Series in Psychology* for this most important volume.

*Brown University.* LEONARD CARMICHAEL.

TROLAND, LEONARD T. *The Fundamentals of Human Motivation.* New York: D. Van Nostrand Company, Inc., 1928. Pp. xiv+521.

This book represents the most serious and detailed attempt to dig to the roots of the psycho-physiological processes concerned in

human motivation that we possess. There are some five hundred pages of compact text. About half is given to the author's conception of the manner of functioning of the nervous system with specific reference, of course, to the physiology of motivation. The other half presents his views of the relation of the mental to the physiological; to a restatement in psychological terms of the physiological theory of motivation previously expounded; and to a far flung exploitation of the combined psycho-physiological theory to various problems of motivation, such as the pleasures of novelty, and the rôle of instincts, emotions, desire, purpose, sex, complexes, personality, character, temperament, etc. in human conduct. The theory is definite, and the supporting discussion acute. In addition the text is rich in apt comment on the various physiological and psychological concepts that pass in review during the development of the central theory—in particular, the historical concepts of hedonism.

The reviewer will confine himself to an account of the central theory. It is only fair to say, however, that the cogency or non-cogency of it can be adequately evaluated only by a detailed reading of the entire book. It is by no means easy reading. Bewildered undergraduates in advanced courses of psychology have attested to that. But no one can lightly dismiss or lightly accept the theory without fully digesting the author's views of cerebral mechanisms, and of the adequacy with which the theory solves such specific problems in human motivation as were listed in the preceding paragraph.

The theory may be characterized as a psycho-physiological hedonism. Essential to its comprehension is the author's analysis of the ambiguities of historical hedonism, with his resultant classification of hedonisms designed to remove the ambiguities. Certain writers (e.g., Aristippus, Spencer, Bain) have asserted that pleasure determines what we do and displeasure what we avoid doing, i.e., pleasure is the basic motivating drive. Others (e.g., Wm. McDougall, Watson, Titchener) assert essentially that pleasure has little or naught to do with determining action. Such divergence of opinion, says Troland, suggests "either that they are not considering the same propositions, or that they are trying to explain entirely separate sets of facts."

A temporal classification of hedonisms is offered to resolve the ambiguities and reconcile the divergencies of opinion. We may speak, in short, of a hedonism of the future, a hedonism of the present, and a hedonism of the past. The first—hedonism of the

future—asserts that action is determined by an anticipated pleasure, e.g., the acceptance of an invitation to dinner. This is an illegitimate hedonism, since a pleasure not now present cannot operate as the cause of a present act. A present intellectual judgment is substituted for pleasure, the actual feeling at the time being possibly neutral. Furthermore, such a hedonism cannot account for animal motivation, since animals do not form anticipatory judgments—and any valid theory of motivation should cover both animal and human choices.

A hedonism of the present seems to offer a way out. The present idea or anticipation of a possible future pleasure may itself be so strongly saturated with actually present pleasure as to determine present behavior. This formula would also let in the animals, for a present tingeing of operating kinaesthetic patterns would alone suffice to release action, without any ideational representation of consequences—the fox hound on the trail of unseen quarry. But, without denying that present pleasure attached to the representation of a future act determines much of our behavior, this hedonism of the present cannot always apply. Many acts are affectively neutral and imply, therefore, some drive dissociated from any present pleasure. The basic objection is, however, that the theory would place us at the mercy of the intensest present pleasure; but it is obvious that in the competition among different degrees of present pleasure the less pleasant—or the positively unpleasant—often conditions action. One persists in the wholesome but painful exercise of limbs that have been sprained, and rats do cross electrified grills for food. A hedonism of the present cannot account for such behavior.

There remains, then, the hedonism of the past. Past pleasures have, of course, been assumed by the two varieties of hedonism just discussed. But the query is now, is there any sense in which past affections may be conceived to have summed themselves so that an algebraic preponderance of experienced pleasure will determine what we shall now do, and a similar overplus of past displeasure what we shall not now do? If so, present affectively neutral acts (e.g., mechanized habit) would be accounted for, as well as present unpleasant acts. Animal behavior would also be fully covered. A hedonism of the future, or of the present, would no longer be relevant. Troland's theory of motivation is fundamentally a hedonism of the past.

Obviously the problems cannot be solved introspectively. One does not revive in present introspection the varying degrees of

pleasure vs. displeasure attached to a given form of behavior throughout the years and then act in accordance with an introspectively explicit algebraic resultant. The whole problem must be restated and solved in physiological terms, since only in the nervous system can a temporal summation of past affections effectively occur. About half of Troland's book is given to the elaboration of such a *physiological* hedonism of the past. Central in the discussion is his concept of "retroflex action."

As an approach to this central concept processes in the organism are classified into beneceptive, nociceptive, and neutroceptive processes. Beneceptive are those that are normally beneficial to the organism—that have (biologically) a survival value, or (physiologically) a preservation or enhancement value for bodily tissue. Introspectively they are usually pleasant. Nociceptive are those processes that threaten survival or are harmful to the tissues. Neutroceptive are those neither beneceptive nor nociceptive—e.g., those initiated chiefly by distance receptors and concerned, inter alia, in recording the environment. They do not report on the welfare of the individual, and they operate in the service of the noci-beneceptive processes. There must be, next, some neuro-muscular mechanism that perpetuates beneceptive and checks or abolishes nociceptive activities. This is "retroflex" action.

We are, of course, dealing here with a concept long familiar—the effects on future behavior of the results of past behavior. One thinks of "satisfiers," "annoyers," and the "law of effect." But such terms sound psychological and we are interested for the present only in the physiological and, moreover, seeking a precise neural mechanism by which past effects *are* utilized for future action. Since "cortical adjustors" are chiefly concerned this retroflex activity has to do with the influence of reactions that have occurred on the facilitation or inhibition of the cortical processes involved. "Nociception is accompanied by a decreasing of the conductances of operating cortical adjustors; beneception is accompanied by an increase of the conductances of the operating cortical adjustors." The former is termed "negative retroflex action" and the latter "positive retroflex action." Positive r. a. *cumulatively* reinforces activities having survival value (beneceptive) while negative r. a. cumulatively inhibits reactions having non-survival value (nociceptive).

Retroflex action is thus the physiological selective mechcanism determining behavior. The memory of an end-pleasure or satisfiaction would not do this, an organic instigating stimulus would not

do it—since both operate only as negative drives, in no way determining positively which inter-current or preparatory reactions are the correct ones in progress towards a consummatory response. An immediate back-kick that influences *operating* cortical adjustors is necessary. Consider, for instance, animal learning. The reason for the animal's restlessness and trial and error behavior is that each succeeding stimulus-response process is inhibited because it does not produce the beneceptive termination demanded by the instigating stimulus, i.e., nociception checks the operating cortical conductances. On the other hand, the final food-securing (beneceptive) reaction increases the immediately preceding (operating) cortical conductances. There is, of course, as in any process of conditioning, a temporal limit beyond which this immediate influence of positive and negative retroflex activity will not obtain. For an exposition of the precise manner in which retroflex action selectively fixes right reactions and eliminates wrong reactions back over the whole inter-current series of reactions (e.g., in maze learning) the text should be consulted (Chapter XIII).

Retroflex action consequent to nociceptive and beneceptive processes is, then, the basic physiological mechanism in motivation. The reviewer cannot pretend to do justice in this brief account to Troland's exposition; he has merely outlined the direction of the argument. He omits, for instance, discussion of the wider extension of the principle of retroflex action into complex conditionings of behavior. But if retroflex action can explain learning it can explain motivation in general. The generalized physiological statement becomes: present choices (reactions) are determined physiologically by the algebraicly summed retroflex effects (positive and negative) of past beneceptive and nociceptive reactions (in number, durations, and *rates*).

A complete account, however, involves a psychological formulation, in terms of a hedonism of the past, as well as the foregoing physiological formulation. First, pleasure—displeasure constitute a linear scale of affective intensities—positive affective intensities above a neutral point, and negative affective intensities below. Next, the linear scale of affective intensities correlates closely with a similar scale of intensities of beneceptive—nociceptive processes. Finally, both scales correlate closely with degrees of positive and negative retroflex effect—positive retroflex effects (degrees of increase in cortical conductances) above the neutral point, and negative retroflex effects (decreases in cortical conductances) below.

Beneceptive processes are, in short, normally pleasant, and nociceptive normally unpleasant. A psychological (hedonistic) formulation of the law of motivation thus becomes possible: the number, durations, and *intensities* of past positive and negative affections (pleasures—displeasures) sum themselves algebraicly and determine present acts.

In the physiological statement the reviewer has italicized the term " rates," and in the psychological formulation the term " intensities." The rate of occurrence of positive retroflex action (increase of cortical conductances) constitutes, that is, the physiological correlate of positive affective intensity (pleasure) and the rate of negative retroflex action (decrease in cortical conductances) the corresponding correlate of negative affective intensity (displeasure).

So much for an account—highly abbreviated and surely inadequate—of the author's retroflex theory of motivation. Critical evaluation of its cogency is not the reviewer's purpose. Many reflections naturally arise. First and foremost perhaps the query, to what useful degree has Troland's exposition introduced detailed precision, defendable by accepted physiological findings, into the undoubted proposition that the *effects* of reactions have much to do with determining later responses to the same stimulus situations? There surely is an effect that may be termed " retroflex " and it must operate on " cortical conductances," whatever these may precisely be. The " law of effect " itself involves such assumptions. But we need to know much more from physiology as to just what cortical conductances are and just what the organic effects of a muscular reaction are, and how they work back to influence the conductances, before we can envisage the mechanism of " effects " in precise detail. A fundamental problem would be, for instance, to devise an experimental physiological procedure by which the effect of reactions on the refractory phase of the neuro-muscular mechanism concerned in the reactions could be determined. Many complexer reactions undoubtedly become more and more refractory, and others less and less refractory to repetition (youthful reactions pall with the years; mah jong gives way to bridge). It would advance knowledge a lot if someone could show in a simple neuro-muscular situation what effect a demonstrable nociceptive or beneceptive organic result of a given reaction would exert on its refractory phase.

Troland has made a careful exploration of available physiological knowledge in the interests of a closely reasoned theory of motivation. Whether or not one fully agrees, he has added reflective pre-

cision to the problem of the effects on further behavior of preceding reactions.

*Yale University.* ROSWELL P. ANGIER.

FISHER, V. E., *An Introduction to Abnormal Psychology.* New York: Macmillan, 1929. Pp. x+512.

In the preface the author states this volume to be intended as a text for a first course in abnormal psychology. After discussion of the normal individual, the reader is oriented to the subject in three chapters describing behavior mechanisms generally, and patients' utilization of them in abnormal behavior. The neuroses and psychoses are then considered in turn, followed by chapters on sleep, dreams, hypnotism, with a closing one on the feeble-minded.

The author's starting point is that personality is the "sum total of the individual's mental aspects," and in the abnormal individual this integration has been impaired. Abnormality differs from normality not in kind, but in degree of deviation from the norm. Reactions in abnormal behavior may be classified into "persistent nonadjustive" ones; regressions; compensatory behavior; inhibition; repression; dissociation. Neurasthenia, psychasthenia, hysteria, and the sexual neuroses are specific manifestations of these reactions, and some illustrative case history material is presented. The same thesis is maintained in presentation of paranoia and schizophrenia, again with some illustrative cases, and is also attempted in the affective psychoses.

The first criticism of the text is the meager recourse the writer appears to have had to the work of even the more important psychiatrists. "Adolph" Meyer—who like Donald Paterson is perhaps by now desensitized to the misspelling of his name—is cited once; Bleuler but three times; Hoch twice; Kraepelin three times. On the other hand there are no fewer than twenty-five indexed references—and the reviewer has counted some not indexed—to McDougall, on whose "Outline of Abnormal Psychology" the author leans very heavily. "It's McDougall this, and McDougall that," the refrain having the regularity of Kipling's Tommy, and sometimes the monotony of his "Boots." In fact, practically all mental disorders are referred to a malfunctioning of the self-assertion and submission tendencies of McDougall. Of citations by the author of his own case history material there is but a modicum. The interpretation of some of the material cited would, further, certainly be questioned by many competent clinicians. Thus the case of the

child (p. 103) who vomited at school-time would, in some well-regulated clinics, certainly not be understood as one of compensation for an inferiority complex. Identification (p. 111) is interpreted as a compensatory mechanism; the psychoanalytic concept of the introjection of the "father precipitate," as a basis for character formation (Glueck), deserves discussion in this connection, irrespective of one's judgment on it, but it is not mentioned. The theoretical conclusion drawn (p. 294) concerning the etiology of the affective psychoses hardly follows from the one case cited from McDougall. There is no mention of MacCurdy. Nothing is said in this chapter of the theories of the manic phase as wish-fulfillment, or of the depressive as based on guilt consciousness.

In his attitude towards Freud the author attempts to steer a middle course: he is restrained in his criticism, while crediting him with opening up the subject of sex as an etiological factor; with pointing to the unconscious; and developing the concept of repression. It cannot be said, however, that the author does justice to Freud's important contributions to the study of the neuroses. At the conclusion of two chapters of discussion of neurotic phenomena, all we find about Freud on this point is three lines. He is cited at somewhat greater length in the consideration of hysteria, and the sexual neuroses. It is germane to note here that much of the presentation of case material referred to the self-assertion and self-submission tendencies, is in line with Alfred Adler's thought, but the author barely mentions Adler.

The merits of the volume include several discussions of topics in refreshing and stimulating fashion. Among these is the explanation of regression (p. 93); the suggestion of the term "over-reaction" for "over-compensation," (pp. 109–110); inhibition (p. 129ff); emphasis on the interplay of both environmental and hereditary factors in the etiology of mental disorders, rather than accepting either class as exclusively or mainly causative; the interpretation of mental disorders as disturbances of the personality as a whole. Unquestionably the high spot of the book is Chapter X, on "Hysteria and Multiple Personality." The bibliographic references at the end of each chapter will be very helpful to the beginning student, although here too it is to be regretted that there is not broader scope, especially from psychopathology. The glossary of terms at the end of the book should also prove helpful. In general, the volume will meet the beginner's need for a descriptive orientation into the functional disorders. It is better for the neuroses than for

the psychoses. Its usefulness is narrowed in proportion to the author's narrowing of his sources, and his too great dependence on McDougall. For the same reasons the advanced reader will find it inadequate.

*Boston Psychopathic Hospital.* SAMUEL J. BECK.

LASHLEY, K. S. *Brain Mechanisms and Intelligence. A Quantitative Study of Injuries to the Brain.* Chicago: Univ. of Chicago Press, 1929. Pp. xiv+186 (with 11 plates).

The author has brought together all of his experiments on the effects of cerebral extirpations on learning and retention in the rat. These very numerous and classically exact experiments have definitely shown that neither learning or retention is dependent upon any specific structure of cortical anatomy but, rather, that both are dependent upon the total mass of structure present. From this the author draws the implication that the existing theories of learning and also of intelligence which depend upon the establishment of particular pathways, or synaptic connections or brain areas cannot be true. "The whole implication of the data is that the 'higher level' integrations are not dependent upon localized structural differentiations but are a function of some more general, dynamic organization of the entire cerebral system."

In place of the older theories, the author outlines a new hypothesis. From a consideration of the work of Kappers and Child, it would seem that "the course of development seems to involve an ever increasing complexity of interacting gradient systems, becoming more delicately balanced and plastic with each step in structural differentiation. It would seem strange if these mechanisms, which contribute so largely to structural organization, should cease to function with the completion of gross development." "The problem of reaction to relations seems to resolve into that of the responsiveness of the final common path to ratios of excitation or to a gradient of excitation between two or more adjacent neurological fields." The author's theory is based on three assumptions (1) "the excitation of a center results in the establishment of a potential difference between that center and other neural aggregates"; (2) "the excitation of a nerve cell is conditioned by the intensity and direction of current in the electric field within which it lies"; and (3) "the spacial distribution of potential differences within the nervous system corresponds somewhat to the distribution of excitation upon the receptor surfaces."

Applying these postulates to the problem of a discrimination reaction, we may assume that a given ratio of stimulus intensities on two peripheral points establishes a potential difference between corresponding points in the cortex. The direction of polarization and the steepness of the gradient will remain constant in spite of considerable alterations in the absolute positions of the centers of excitation. Cells subject to the influence of this system will have their excitability modified in a constant direction so long as the properties of the gradient field are constant.

*University of Pennsylvania.* SAMUEL W. FERNBERGER.

YERKES, ROBERT M. and ADA W. *The Great Apes.* A Study of Anthropoid Life. New Haven: Yale Univ. Press, 1929. Pp. xix+652.

The present volume brings together everything that is worth knowing about the anthropoids at the present time. The author first spends some pages in historical survey in an attempt to work out terms and relations within the group. Part II gives the little that is known about the gibbon. Here, as in succeeding chapters, are discussed the historical background, structural characteristics, distribution habitat and species, habits of locomotion, diet and life history. In this section on the gibbon are also recorded observations with regard to affectivity, voice and the like but one is impressed with the scarcity of experimental material to be found on this ape.

The section on the orang-outan is much fuller both of observational and experimental studies. We have a further increase in available material for the chimpanzee to whom over 150 pages are devoted. One very important section for future investigators is an interesting account of the hygiene and care of this ape. When one brings together the observations and experiments of Köhler, Kohts (for the first time adequately summarized in English) and of Yerkes and his associates, the basis has been laid for a fairly complete understanding of the life, habits and mentality of this anthropoid ape. A similar section is devoted to the observational and experimental studies of the gorilla and here one finds, largely due to the work of Yerkes himself, the basis for a reasonable understanding of this ape although the number of individual gorillas studied has been necessarily small.

A final section is given to comparisons and conclusions. The four varieties of anthropoid are compared with regard to structural form, mode of life, life history, receptivity and intelligence. This

material is tabulated in exceptionally complete and usable form. There is a very complete bibliography and index at the end of the volume.

The present volume is a monumental work which must be of great value to any scientist interested in anthropoid behavior and in the relations of such behavior with that of man. The volume is handsomely gotten up with a great many excellent photographs. It is interestingly written and the treatment as well as the nature of the material should appeal to the intelligent layman as well as the scientist.

*University of Pennsylvania.* SAMUEL W. FERNBERGER.

PIÉRON, HENRI. *Le Développement Mental et L'Intelligence.* Paris: Félix Alcan, 1929. Pp. xii+95.

This small volume includes four lectures presented by Piéron at the University of Barcelona, embodying a sketchy presentation of the high lights of mental development and of problems of mental measurement.

The first lecture is devoted to a discussion of stages of mental development, starting with a brief description of the native behavior endowment of the child. From this, the author proceeds to a description of other stages of development; to a consideration of the relationship between physical growth and mental development; of physiological factors in growth; stages of affective development, and related topics of significance in individual development.

In the second chapter, including the second of his lectures, a discussion of individual differences in development serves as an introduction to a description of the work of Binet and others in setting up norms of development for the various age levels and in preparing tests for determining the extent to which a child has developed in relation to these norms. Elements of error in measurement, variability at different age levels, the age at which development ceases, and similar questions are referred to by the author in the course of this discussion.

The first part of the third chapter is given over to pointing out the advantages of determining early the level and rate of intellectual development of children. The remainder of this chapter, and practically the whole of the fourth and final chapter in this little volume, include a critical discussion of the concepts of general intelligence, the nature and significance of the I.Q., the weaknesses of standard scales used in the measurement of intelligence, etc. Particular stress

is laid upon the importance of avoiding global evaluations on the basis of an I.Q. and upon the need of analytic studies of individual traits by means of profile and related methods.

This small volume by Piéron seems to embody all that can be expected in a sketchy presentation of mental development and of problems of mental measurement. It is sketchy without being incomplete, and critical without being destructive. One can even enjoy the sly digs at American investigators, and can excuse the slighting of more important American investigators in the field of mental testing in the interest of less important French and other Latin studies as a perhaps necessary procedure in the dissemination of French culture in the nearby Latin countries of Europe.

*University of Pennsylvania.* MORRIS S. VITELES.

WEISS, ALBERT PAUL. *A Theoretical Basis of Human Behavior* (Revised Edition). Columbus, Ohio: R. G. Adams & Co., 1929. Pp. xvii+479.

In this revision of his explicit formulation of the behavioristic position, Professor Weiss has "tried to profit by the many criticisms directed against the 1925 edition." The chapter divisions remain the same. Sections transcribed from the first edition are amplified by new paragraphs, there are minor changes in wording, and new treatments of habit, feeling, and meaning. The fundamental tenets remain unaltered.

Utilizing as he does the postulates of physical science as the basis for the study of human behavior, Professor Weiss has been forced to notice the skepticism of some modern physicists to a thorough-going mechanism. He chooses, however, to dismiss such popularizers as Milliken, Lodge, Whitehead, and Pupin, rather than to answer them. "Should a physicist whose whole lifetime has been devoted to the study of physics assume the authority of instructing others on the development of human behavior" (p. 55)? One suspects that the physicists might be tempted to reply, "Should the student of human behavior reprimand the physicists for spoiling the kind of physics he wants for his behaviorism?"

Lloyd Morgan's emergent evolution is discussed in relation to mechanism. Granting the unpredictable qualities of "emergent" structures, Weiss contends that the limitation is simply one of inadequacy in present formulated laws. "Since the individual under social conditions is in a different environment than when alone we would expect differences of behavior. That we actually get them

is no indication that the changes are non-mechanical. The forms of behavior were simply not predictable from the laws that had been developed" (p. 96).

The new section on habit formation follows Max Meyer's principles of double stimulation and deflection as the neural conditions for elimination and substitution of reactions (p. 204). The treatment of meaning which is new to the book fills two pages. "For the behaviorist the meaning of a stimulus is defined by the responses that are made to it" (p. 325). The dictionary meaning is one series of many possible responses, the one which is biosocially accepted. The analysis of feeling and affection might be selected as a point of attack by those who would see subjectivity filtering into the rigidly objective system. "While the taste of sugar is pleasant, we cannot say that pleasantness is located in the mouth. The pain of a toothache may be localized in the tooth, but the unpleasantness of the pain is not in the tooth. The unpleasantness is merely an implicit reaction to the extent with which the reactions to the tooth interfere with our ordinary routine activity" (p. 420).

Most scientists today have not recognized the possibility of studying human achievement without the conception of mind. "In this sense modern behaviorism is more than a revival of Materialism or Positivism. It definitely challenges traditional psychology to show that the concept of mind (or its historical equivalents) is necessary for the scientific study of human achievement" (p. 304).

*Yale University.* ERNEST R. HILGARD.

GARRETT, HENRY E. *Great Experiments in Psychology.* New York: Century, 1930. Pp. x+337.

The volume is not a tribute to isolated experiments and experimenters of the past; it is not a laboratory manual; it is not a handbook or reference book. The great experiments are, to be sure, related to the eminent men who performed them, and the biographical note is furthered by full-page portraits of Binet, Terman, Ebbinghaus, Pavlov, Thorndike, Woodworth, Watson, Galton, Cattell, Köhler, and Fechner. Others whose names appear in the chapter titles as figuring in the great experiments are Cannon, Weber, Franz and Lashley. The great experiment merely introduces the field, and the subject-matter is brought up to date. Thus Terman moves on from Binet, and much of the Ebbinghaus chapter is devoted to Luh. The book is elementary enough to be read by a beginning student not yet familiar with psychological jargon, and

as a supplementary or second book to be used in connection with a standard text it is a unique and important addition to the resources for the first course in psychology.

The exposition is lucid throughout. The tables and charts have been reconstructed from the originals, and are all strikingly understandable. The basic facts, e.g., of the psychophysical methods, are presented and illustrated so that the beginner can understand the principles without being confused with detailed mathematics or controversial points. The simplification is cautious, and not at the expense of accuracy. A commendable restraint is shown in the suggested readings at the end of each chapter, there being in no case more than four references. These give the original source and the best summaries of the more recent experimental work in the field under consideration.

The discursive style limits the actual number of specific experimental findings to rather less than are crowded into many textbooks, and I am inclined to think that the volume has few advantages over a good textbook for use in an experimental or laboratory course.

This is the second book in the Century Psychology Series edited by R. M. Elliott.

*Yale University.* ERNEST R. HILGARD.

# A NOTE ON THE RELATION BETWEEN BRAIN INJURY AND RETENTIVENESS

BY ARTHUR W. MELTON

*Research Associate, American Association of Museums*

In K. S. Lashley's "Brain Mechanisms and Intelligence" there is a chapter entitled, "The Influence of Brain Injuries Upon Retentiveness," which demands comment because of the experimental and theoretical possibilities it suggests. Without doing violence to Lashley's argument, the findings reported in that chapter may be summarized briefly. (1) "For Maze III (the most difficult maze used, that of eight culs-de-sac) there is a clear indication that the animals which learn slowly tend also to make poor scores in retention tests. This is further indicated by the correlations between extent of brain injury and the practice required in retention tests. Those correlations for the different criteria are $p_{time} = 0.13$, $p_{errors} = 0.51 \pm 0.11$, $p_{trials} = 0.57 \pm 0.10$." [1] (2) These correlations are approximately those obtained for the relation between extent of brain injury and ability to learn, consequently some measure of the degree of retention for the normal and operated groups relatively freed of the factor of ease of learning during relearning was necessary. Lashley found that "the animals with brain lesions not only required a great deal more practice, measured in absolute terms for all three criteria of learning (time, errors, trials), for reacquiring the habit than did normal animals, but also that they required relatively more practice in proportion to their initial learning records";[2] and that, "On the first trial of retention tests the operated animals required almost four times as much time and made four times as many errors as normals which had reached the same grade of efficiency in initial training," [3] in spite of the fact that the injured rats ran the maze nearly as quickly on errorless trials as did the normal rats. (3) Lashley concludes that there is a reduction in retentiveness for a complex maze habit following cortical injury.

[1] Lashley, K. S., *Brain Mechanisms and Intelligence.* Chicago University Press, 1929. P. 77.

[2] *Idem*, p. 79.

[3] *Idem*, p. 79.

That Lashley has measured retentiveness per se is questionable. The objection lies in his experimental procedure. The procedure which gave data leading to the conclusion cited above may be summarized in the following manner:

*Experimental Procedure*

| *Control Group* | *Experimental Group* |
|---|---|
| I. No operation | Operation with removal of varying amounts of cortex. |
| II. Learn 8 culs-de-sac maze | |
| Learn 3 culs-de-sac maze | Activities interpolated between learning and retention tests on the 8 culs-de-sac maze. |
| Learn 1 cul-de-sac maze | |
| Learn brightness-discrimination | |
| *Interval of about 40 days* | |
| III. Relearn 8 culs-de-sac maze | Activities interpolated between learning and retention tests on the 1 cul-de-sac maze. |
| Relearn 1 cul-de-sac maze | |
| Relearn brightness-discrimination | |

The procedure outlined in the above table can give nothing other than ambiguous data upon the problem of retentiveness. The history of the experimental investigation of retention shows that there evolved at a very early date a dichotomy of experimental method. On the one hand, Ebbinghaus studied the retention of lists of nonsense syllables and stanzas of poetry after set intervals of time during which there had been no additional memorization of similar nature.[4] The only activities interpolated between the learning and retention tests were those which made up the common everyday life of the individual. The work of Müller and Pilzecker introduced a second methodological approach to the study of retention.[5] They interpolated between the tests for learning and retention of a primary list of nonsense syllables the memorization of an additional list or the studying of pictures. It was discovered that this interpolated activity led to a greater difficulty in relearning the primary list of syllables than would have been the case if nothing other than everyday activities had intervened. To this influence of the interpolated activities they gave the name "retroactive inhibition."

Since these early investigations the dichotomy of method for studying retention has been adhered to. Finkenbinder, Bean, Radosavljevich, Luh, and others have investigated the conditions of reten-

[4] Ebbinghaus, H., *Über das Gedächtnis*, Leipzig, 1885.

[5] Müller, G. E. and Pilzecker, A., Experimentale Beiträge zur Lehre vom Gedächtnis, *Zsch. f. Psychol.*, 1900.

tion when there has been no interpolation of additional learning activity other than that involved in the normal adjustments of the individual to the environment. R. Heine, Robinson, Webb, DeCamp, Tolman, Skaggs, McGeoch, and others have investigated the influence of interpolated activities upon retention, that is, they have studied " retroactive inhibition " using the procedure first introduced by Müller and Pilzecker.[6]

Our first criticism of Lashley's work is that he has not set up his experiment in such a way as to demonstrate the loss of maze habits which would have occurred because of reduction in retentiveness as such. He has introduced the factor of retroactive inhibition into his experiments by the experimental series in which the learning of the maze of three culs-de-sac, the one cul-de-sac maze, and the brightness-discrimination habit is interpolated between the original learning of the eight culs-de-sac maze and the relearning of that maze. The learning of definite behavior patterns is also interpolated between the learning and the retention tests on both the one cul-de-sac maze and the Yerkes discrimination box. The obvious assumption which Lashley makes is that since the activities are interpolated between the learning and retention tests for both the normal and experimental (operated) groups, whatever differences in performance appear must be due to the alteration of the factor of retentiveness. This in turn implies either that susceptibility to retroactive inhibition is not influenced by cortical damage, or that there is a high positive correlation between susceptibility to retroactive inhibition and retentiveness. Neither of these implications have definite experimental proof.

Not only does the experimental procedure obviate any determination of the effect of brain injury upon retentiveness as such; it defies any interpretation of the data as regards the problem of susceptibility to retroactive inhibition. In work on retention in man the factor of retroactive inhibition has been found to vary, among other things, with the degree of learning (overlearning) of the primary activity, with the amount of interpolated activity, with the degree of similarity of the interpolated and original activities, and

[6] One may verify the claim of a dichotomy of method by referring to reviews of the literature on Forgetting, to be found in: C. W. Luh, The Conditions of Retention, *Psychol. Monog.*, 1922, **31**; E. S. Robinson, Some Factors Determining the Degree of Retroactive Inhibition, *Psychol. Monog.*, 1920, **28**; E. S. Robinson, Memory, *Psychol. Bull.*, 1924, **21**; J. A. McGeoch, Memory, *Psychol. Bull.*, 1928, **25**; J. A. McGeoch, Memory, *Psychol. Bull.*, 1930, **27**.

to some extent with the temporal relations of the interpolated and original activities. In Lashley's work there is more than one interpolated activity; the interpolated activities are not the same between the learning and retention tests on the eight culs-de-sac maze, the one cul-de-sac maze, and the discrimination box; and the degree of similarity of the original and interpolated activities varies within unknown limits. In pointing out these discrepancies in Lashley's procedure, we have assumed that the same factors operate in retention and forgetting in both the white rat and man. The carrying over of results on the problem of retroactive inhibition obtained with human subjects to a critique of animal work needs apology, but such a procedure leads to a precise positing of certain experimental problems and furthermore it is substantiated somewhat in this particular instance by the work of Webb.[7]

It may be objected that Lashley's comparisons are always between the performance records of the control group and those of the experimental group. Such an objection is valid if we accept a broad definition of retentiveness as that complex of factors, both organic and environmental, which makes for a loss in the capacity for reinstatement of a previously learned activity; however, such a comparison fails if we are interested in the specific mnemonic factors which vary with cortical injury, and if we intend to make such potentially significant data coordinate with the mass of experimental work on human retention, thus fulfilling one aim of comparative psychology and neurology. This latter interest may be satisfied only by eliminating the factor of interpolated activities while keeping constant or varying those factors which lend potency to their influence upon the subsequent reinstatement of the original learned activity. The most obvious first step in such a program would be to compare the retention of a maze in two groups of operated animals (with varying amounts of cortical destruction), one group having no special interpolated learning activity and the other group having interpolated learning, and in two groups of unoperated animals, one group having no interpolated learning and the other group having interpolated learning.

Although Lashley's conclusions are of doubtful validity, the data presented by him are suggestive. In noting the qualitative differences in behavior with increasing amounts of cortical destruction

[7] Webb, L. W., Transfer of Training and Retroaction. *Psychol. Monog.*, 1917, 24, No. 104. Pp. 90.

Lashley observed that the rats with the more severe lesions showed a marked tendency to the perseveration of specific part responses in learning the mazes.[8] This, combined with an increase in the rate and amount of activity, caused the rats to repeat the same maze error many times in succession. Thus an extensively injured rat would enter and reenter a blind alley within any one trial much more frequently than would those rats with smaller lesions or with no lesions at all. It may be that this increased occurrence of identical errors is due to something more than a combination of increased activity and chance factors. It is significant that in those rats which showed the greater tendency to perseveration of sensori-motor response there was, roughly, a greater degree of cortical destruction and a greater loss in "retentiveness" which might be interpreted as a greater susceptibility to retroactive inhibition.

It is possible that a relation exists between the degree of susceptibility to retroactive inhibition, the tendency to perseveration of response, and cortical mass or extent of cortical injury. It has been previously noted that there is probably a fundamental relationship between learning ability and refractoriness to the repetition of an identical response.[9] An abnormal manifestation of such a relation is found in the ecolalia of idiots and children. Some interesting questions, subject to experimental attack at either the human or infra-human levels, may be posited. Does the degree of susceptibility to retroactive inhibition increase with a decrease in intelligence? What is the experimentally obtained correlation between learning ability, degree of susceptibility to retroactive inhibition, and the degree of refractoriness to repetition? In sub-human species, what is the relation between cortical mass and retentiveness, that is, retention when specific activities have not been interpolated between the learning and retention tests? And again, what is the relation between the degree of susceptibility to retroactive inhibition, the tendency to perseveration of response, and the extent of cortical injury? And finally, with reference to the functional anatomy of the nervous system, since Lashley's most extensive lesions often involved destruction of portions of the basal ganglia, what are the possibilities that the lack of refractoriness to repetition of response

[8] Lashley, K. S., *Brain Mechanisms and Intelligence*, pp. 117–118.

[9] Dodge, R., *Elementary Conditions of Human Variability*, p. 72, Columbia University Press, New York, 1927; Thorndike, E. L., The Refractory Period in Associative Processes, *Psychol. Rev.*, 1927, **34**, 234–236; Dodge, R., A Note on Professor Thorndike's Experiment, *Psychol. Rev.*, 1927, **34**, 237–240.

has a more fundamental relation to basal ganglia function than to cortical function?

These questions are only illustrative of possible points of departure in the psycho-biological study of the phenomenon of retention. For a fundamental method of study, Lashley's work is of prime importance; but a refinement of his experimental procedure in the light of the historical distinction between the problem of retention and the problem of retroactive inhibition must be introduced if the findings in this particular field are to be made coordinate with those already amassed by the psychologist.

If such a program of research should demonstrate a relation between the degree of susceptibility to retroactive inhibition, the tendency to perseveration of response, and cortical mass or extent of cortical injury, as we have postulated after an examination of Lashley's data, it would be of immense importance for the understanding of the phenomenon of forgetting at different levels of the phylogenetic and ontogenetic scales. Not only would such a correlation give suggestions valuable for the solution of the problem of human forgetting and of the individual differences therein, but it might well serve as an indication of problems in the functional anatomy of the nervous system, especially the cortex.